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On the Construction of Wrought Iron Lattice Girders.

By THOMAS CARGILL, C. E.

(Continued from page 21.)

In the examples given of the flanges of double lattice girders, the connecting medium between the tension and compression bars composing the web, and the plates which are a portion of the flanges, has been supposed to consist of two pairs of angle-irons, riveted longitudinally to the plates throughout the whole length of the girder. In some instances tee-irons are employed as the means of attachment, each pair of longitudinal angle-irons being replaced by one tee-iron. The table of the tee-iron is riveted to the plates in the ordinary manner, and the bars of the web are attached to the tongue or rib, on the outside and inside successively. This substitution is well adapted for girders of small spans, but cannot be used with equal advantage in those of large dimensions. The riveting of the tee-iron through its table portion to the plates is better and more efficient than that of two angle-irons of the same sectional area. If we suppose the centres of the rivets to be the centres of resistance, the resultant of the forces in the former case will pass through the centre of gravity of the tee-iron, and the leverage will be reduced to a mere nothing. On the other

hand, considering each angle-iron in the same pair to act independently of the other, the leverage will be equal to $\frac{l}{2}$, taking l to represent the length of the horizontal side of the angle-iron. To a certain extent each pair of angle-irons does act as one, and as the points where the small space left between them for the insertion of the web is filled up by the bars and distance pieces they may be regarded in the light of one large tee-iron.

The chief advantage of the pair of angle-irons is in their vertical sides, which can be made as strong and stronger, if necessary, than those placed horizontally. This is not the case with a tee-iron; its rib is its weak part, and in the sections usually rolled is never equal in area to that of the table part. The bars of the web never obtain in their attachment to the tee-iron the same hold which is afforded to them by being riveted in between a pair of angle-irons. Moreover for girders of large span, the necessary amount of sectional area to be given to a pair of angle-irons joining the web to the flanges could not be obtained in one tee-iron, unless specially rolled for the purpose; and there are no commensurate advantages attending its use, to warrant the extra expense which would be thus incurred. In small girders, particularly in those which have a single lattice web, and in others where the sectional area of the vertical sides of the flanges is not required to be very large, such as for instance in the cross girders of a bridge, it will be found simpler to use a single tee-iron instead of double angle-irons. The amount of riveting will be exactly the same in both arrangements. The tee-iron affords somewhat greater facilities with respect to the workmanship, and there is a saving of filling pieces effected in consequence of the bars of the web being attached directly to the tongue of the tee-iron on either side. The only packing pieces required will be at the crossings of the bars of the web; and their thickness will be constant and equal to that of the rib of the tee-iron instead of varying inversely as the thickness of the web, which is what occurs where double angle-irons are used.

In the last number an illustration was given of the joint of one of the double angle-irons forming a portion of the flange of a double lattice girder. Fig. 1 and Fig. 1*a* represent in plan and section respectively the joint of a tee-iron in a similar position. The covering pieces consist of the main plate of the flange, which acts as one wrapper over the table of the tee-iron, and two pieces of angle-iron, which serve as the other wrappers to both the table and rib, and complete the joint. In order that the plate should act as an efficient wrapper its thickness should at least equal that of the table part of the tee-iron. It may be greater, but should never be less, as it would be bad workmanship to rivet heavy tee or angle irons to light plates. This remark does not apply to the riveting of upright angle or tee irons to the sides of plate or box girders, but it must be borne in mind that in the latter instance the upright irons are in comparatively very short lengths, and only intended to act as stiffening pieces to the sides, their sectional area

forming no item in the calculations of the strength of the girder. But even in such examples there is a proper limit to the weight of the upright stiffening irons, as may be inferred from what has been already said respecting the riveting together of very unequal thicknesses of iron.

Fig. 1.

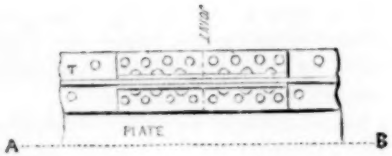


Fig. 1*a*.

It will be necessary in the joint we are now considering that the section of each angle-iron wrapper should equal that of the tee-iron to be jointed. It will be seen on referring to the section in Fig. 1*a* that each angle-iron only covers part of the table of the tee-iron, and has nothing to do with the other half which is covered by the other wrapper. Let A = sectional area of the whole tee-iron and a that of the table; since both the angle-iron wrappers will be of the same dimensions, put A_1 for the sectional area of either, then $A_1 = \left(\frac{2A-a}{2}\right)$. In the ordinary sections of tee-iron the rib is rolled of an uniform thickness with the table, although if necessary it can be made either greater or less. Considering the thickness uniform throughout; putting l for the length of the table, l_1 for that of the tongue or rib, and t for the thickness, we have $A = (l+l_1)t$ and $a = lt$; substituting these values for A and a, we find $A_1 = \frac{(l+2l_1)t}{2}$. Let l_2 and l_3 equal the length of the two sides of the angle-irons measured from out to out, and put t_1 for the common thickness, which it is necessary to find before the size of the angle-iron can be determined upon. We shall have, using this notation, $A_1 = (l_2+l_3-t_1)t_1$, and our equation will be

$$(l_2+l_3-t_1)t_1 = \frac{(l+2l_1)t}{2}.$$

Solving for t_1 we obtain

$$t_1 = \pm\sqrt{-\left(\frac{l+2l_1}{2}\right)t+\left(\frac{l_2+l_3}{2}\right)^2}+\left(\frac{l_2+l_3}{2}\right).$$

From an inspection of the section, $l_2 = \frac{l-t}{2}$, and $l_3 = l_1$.

Let us take *e.g.* a tee-iron of the following dimensions, $5'' \times 4'' \times \frac{1}{2}''$, that is, let $l = 5''$, $l_1 = 4,''$ and $t = 5''$, l_2 will $= 2{\cdot}5$, and t_1 will have a

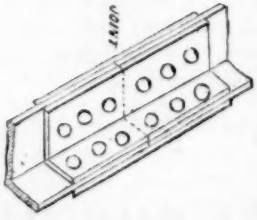
value of ·575″, or about $\frac{9}{16}$″, which will give the required section of metal for the angle-iron wrappers. In some situations, the angle-iron wrappers might be dispensed with, as a piece of plain bar iron riveted to each side of the tongue, and another piece over the table, would be sufficient to secure the joint, the under side of the table being thus left uncovered. As the same number of rivet-holes would have to be punched, and the saving of material in the wrapper would be very trifling, it would be preferable in all cases to employ the arrangement shown in the figure. For bridges designed to carry only a single line of rails, a very light yet at the same time a very strong cross girder may be constructed of two tee-irons forming the upper and lower flanges, jointed together by a lattice web; an ample degree of stiffness will be obtained by using light angle-irons for the struts.

Our iron manufacture has at present arrived at so high a degree of excellence, and the various processes through which plates, bars, and other forms of iron are put, before being fit for girder work, are conducted on so extensive a scale, that the sections of iron required for the web of a lattice girder can generally be obtained rolled in one length. That such was not the case some years ago is evident from the fact that, in the Boyne Viaduct, the bars and angle-irons composing the web are not all in one length, except some of the short ones near the ends of the girders. With these exceptions, the length of the lattice bars is between 33 and 34 feet, far beyond what would be the dimensions in any moderate size girder. It is very doubtful even at present whether it would be advisable to employ rolled sections of iron in such long lengths, although they could certainly be turned out from the bloom in lengths twice as great as the above. There is not the same difficulty in getting long lengths of bar and other sections of iron rolled homogeneously as there is in long plates, nor have the former so much tendency to split into laminæ, or peel under a severe strain, as the latter. The conformation of the fibres, and the mode of working the two kinds of iron, is the chief cause of this difference.

In Fig. 2*a* and Fig. 2 are shown the section and isometric elevation of the joint and wrappers of an angle-iron in the position they would occupy if used in the web of a lattice girder. Both the inner and outer wrappers consist of pieces of angle-iron, one fitting in inside the main angle-irons, and the other covering the joint on the outside. The sectional area of the wrappers will each equal that of one of the main angle-irons, consequently as they will not both be of the same length, the inner one will have a greater and the outer a less thickness than the angle-irons to be jointed. A general formula for finding the thickness of an angle-iron wrapper was given in last number, so that its sectional area should equal that of the main angle-irons. Supposing however, as should always be the case, that the edges of the wrappers are

Fig. 2*a*

Fig. 2.



flush with those of the angle-irons, as in Figs. 2 and 2*a*, the same result may be arrived at in a much simpler manner. Take for instance the inner wrapper in Fig. 2*a*; it will be sufficient to consider one of the sides, as the same thickness will hold for both. Let l=length of horizontal side of the main angle-iron, and t its thickness, l_1 the length of the same side of the wrapper, and t_1 the thickness required, which it will be seen at once, will be greater than t. We thus have $l \times t = l_1 \times t_1$, l and l_1 being both measured from out to out; $l_1 = l - t$, and consequently $t_1 = \frac{l \times t}{l - t}$. The formula for the thickness of the outer wrapper is not quite so simple. Put l_2 for the length of its horizontal side and t_2 for its thickness; then as before $lt = l_2 \times t_2$; but $l_2 = l + t_2$ and $lt = (l + t_2 \times t_2)$, from which we obtain $t_2 = \pm \frac{\sqrt{(4lt + l^2)} - l}{2}$. In jointing angle-irons, instead of using two pieces of the same description of section for wrappers, four pieces of plain bar iron are often employed instead, but it is evident this does not make so firm and compact a joint as the arrangement shown in the figures. Were it not convenient to procure sections of angle-iron suitable for the wrappers, they might be replaced by two pieces of ordinary plate iron, bent to fit inside and outside the main angle-irons as nearly as they could be made to do. It would not be difficult to accomplish this, for the length of the wrappers for joints in sections similar to the above is generally very small compared to that of the wrappers required for joints in other portions of the girder.

We shall consider the joints in the web of which we are now treating to be in compression, as those in tension are simply joints in a plain bar section, which have been already referred to. Let N equal number of rivets required for each side of a joint in an angle-iron acting as a strut in the web of a lattice girder, l and l_1 the length of the two sides of the angle-iron measured from out to out, and the rest of the notation as before, then

$$N \times \frac{\pi d t}{2} = \Big([l + (l_1 - t) - nd] \Big) t, \text{ from which}$$

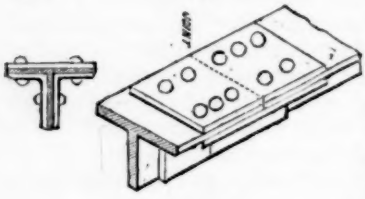
$$N = 2 \left(\frac{l + (l_1 - t)}{\pi d} - \frac{u}{\pi} \right).$$

Except in very long girders the value of n will not exceed 1, and it will depend altogether upon the amount of the variable or moving load, whether it may not be taken equal to zero. If the maximum moving load be not sufficiently great to cause the strut to undergo any severe tensile strain, n may be taken $= 0$, on the assumption that bars and plates in compression are not affected by the insertion of rivets completely filling up the holes. As there would be in the struts so very little difference between the net and gross area, and bearing in mind the nature of the strain they are under, we may consider $n = 0$, and therefore find $N = 2 \left(\frac{l + (l_1 - t)}{\pi d} \right)$. This value for N gives the whole number of rivets required in both sides or webs of the angle-iron on one

side of the joint, half of them being inserted in the horizontal and the other half in the vertical side. In order to render this arrangement of the joint rigidly correct, it is necessary that two conditions be fulfilled on the part of the angle-irons to be jointed: either the thickness of both the horizontal and vertical sides must be uniform, and their length nearly the same, or their difference in length compensated for by a difference in thickness; in other words, $l \times t$ should nearly equal $l_1 - t)t$ or $l \times t$ nearly equal $(l_1 - t)t_1$. It is evident that were this not the case, and that the two sides of the same angle-iron were very dissimilar in sectional area, we should be compelled to design the joint for each side separately. One side would thus require more rivets than the other, and a longer wrapper. The practical inconvenience would then arise of having to cut off a portion of one of the flanges of the wrapper covering the smaller side of the angle-iron, or make the whole cover longer than necessary, and so weaken the joint. The same necessity for an unequal length of sides in the angle-irons forming the compression bars in the web of the girder does not exist as in the longitudinal angle-irons composing a part of the flanges, and in the latter the disparity of size may be kept practically within such limits as to obviate the contingency alluded to. It should always be remembered that it is safer to trust the rivets of a joint with any excess of strain, than the plates or bars, the former being of superior iron, and also better calculated from their position to resist any additional strain than the latter.

A joint of tee-iron is shown in Figs. 3 and 3*a*, consisting of a plate over the outside of the table part, and two angle-iron wrappers on each side of the tongue. This arrangement is nearly identical with that represented in Figs. 1 and 1*a*, the small plate wrapper replacing that portion of the main plates which formed the outside wrapper. The gross section of the small plate will equal that of the table of the tee-iron $= l \times t$. The formula for finding the thickness of the angle-iron wrappers, and consequently their section, will be the same as that given above when the tee-iron was considered as composing part of the flange of the girder. Wherever tee-irons are used of dimensions so small that the tongue portion is less than $2\frac{1}{4}$ inches, it is very much weakened by the insertion of rivets. If such a tee-iron forms one of the compression bars of the web of a girder, it would be preferable to dispense with any wrappers over the joint of the tongue, employing only the plate wrapper over the outside of the table, as seen in Figs. 3 and 3*a*, and two similar smaller wrappers in the inside, which would replace in fact the horizontal sides of the angle-irons shown above. The section of each small plate will equal $\frac{l \times t}{2}$. The tongue is the weak part of a tee- and is intended more to serve as a strength-

Fig. 3*a*. Fig. 3.

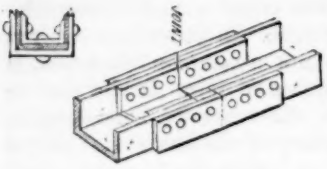


neing and stiffening adjunct to the table part, than to bear any great strain itself. It was mentioned, when treating of the forms of iron suitable for the bars of the web of a lattice girder, that rivets should not be inserted in the ribs of tee-iron unless either actually unavoidable, or where the ribs were of very large size. In jointing a tee-iron in which the table and the tongue have not a common thickness, it will be necessary, supposing the description of joint to be similar to that given in Figs. 3 and 3*a*, if the difference be considerable, to use angle wrappers in which the thickness of the horizontal side is not the same as that of the vertical. Put, as before, $l \times t$ for the length and thickness of the table part of the tee-iron, l_1 and t_1 for the similar dimensions of the tongue, measured inside; let $l_2\, t_2\, l_3, t_3$ be the lengths and thicknesses respectively of the horizontal and vertical sides of the angle-iron wrappers. For the horizontal side of the wrapper we have $l_2 \times t_2 = \frac{l}{2} \times t$. Substituting for l_2 its value $\frac{l - t_1}{2}$ we find that $t_2 = \frac{l \times t}{(l - l_1)}$. If l_3 be measured inside, instead of from out to out, we obtain $l_3 \times t_3 = (l_1 - t_2)\, t_1$; but $l_3 = l_1 - t_2$, and consequently $t_3 = t_1$. It will be sufficiently accurate in practice to make, in the majority of instances, the thickness of either side of the angle-iron wrapper equal to that of the part of tee-iron which it covers. With respect to the length of each side of the angle-iron wrapper, a great deal will depend upon the relative proportion of the tongue and the table of the tee-iron It is manifest that the joint of a tee-iron is open to the same practical inconvenience on this head as that of an angle-iron. In most sections of tee-iron, however, it will be found that $l_1 \times t_1$ nearly equals $\frac{l \times t}{2}$, and therefore there will be no necessity for cutting one side of the wrapper shorter than the other, or weakening by superfluous rivet-holes one portion of the tee-iron at the expense of the other. An angle-iron wrapper, in reality, acts the parts of two wrappers in one piece, and not only covers each portion of the joint belonging to its respective sides, but forms a strong attachment between those portions themselves. It will be seen on referring to Fig. 3*a*, that the angle-iron wrappers evidently strengthen the union between the tongue and the tables of the tee-iron, besides protecting each separate part of the joint, thus causing the whole joint to act together as one piece, in a manner which could not be attained were the two sides of the angle-irons replaced by separate pieces of plate.

A method of jointing a channel iron is represented in Figs. 4 and 4*a*, and consists of covering the joint internally and externally with a couple of wrappers of the same section. The areas will be the same, and consequently their thicknesses will vary, which must be determined before the areas of the different parts, viz: the ribs and channel portions, can be apportioned. Let l= the length on the outside of the

channel irons to be jointed, l_1 the length of the sides or ribs measured on the outside, and t the thickness which will be considered uniform throughout. Let us take the inside wrapper first, put l_2, l_3, and t_1 for its dimensions, measured similarly as above, and we have the equation $(l_2 + 2l_3)t_1 = (l+2l_1)t$. It will be seen on reference to Fig. 4*a*, that $l_2 = (l-2t)$ and $l_3 = (l_1 - t_1)$; substituting these values, we obtain

Fig. 4*a*. Fig. 4.



$$[(l-2t+2(l_1-t_1)]t_1 = (l+2l_1)t \text{ and } (l-2t+2l_1)t_1 - 2t_1^2 = -\frac{(l+2l_1)t_2}{2}.$$

Reducing and solving for t_1 we find

$$t_1 = \pm\sqrt{\left[\left(\frac{l-2t+2l_1}{4}\right)^2 - \left(\frac{l+2l_1}{2}\right)t\right]} + \frac{(l-2t+2l_1)}{4}.$$

If we make our main channel irons $6'' \times 2'' \times \frac{1}{2}''$, we shall find that t_1 = practically $\frac{5}{8}''$, which will give an equivalent section for the inside wrapper; for the outside wrapper, putting l_4, l_5, and t_2 similarly as for the other channel irons, we shall have $(l_4 + 2l_5)t_2 = (l+2l_1)t$. Make the area of the main channel irons, or $(l+2l_1)t = A$, $l_4 = l+2t_2$, and $l_5 = l_1+t$, and finally we obtain for the thickness required

$$t_2 = \pm\sqrt{\left[\left(\frac{l+2l_1+2t}{4}\right)^2 + \frac{A}{2}\right]} - \left(\frac{A}{4t} + \frac{t}{2}\right].$$

In this instance t_2 will nearly equal $\frac{7}{16}''$.

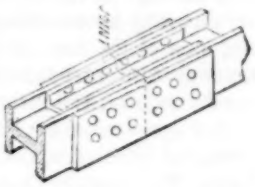
In the figures the channel irons are shown sufficiently large to be able to take rivets in their webs or flanges. This advantage could only be obtained in channel irons of very large sections—far too large for the compression bars of the web of any moderate sized girder; and it is questionable whether it would be advisable to insert rivets in the flanges of any channel irons of the sections usually rolled. Partly on account of the above reasons, and partly from the difficulty in practice of obtaining sections of channel iron suitable for the wrappers—for if they do not fit well it would be better to dispense with their use altogether, as they would only make a loose and insecure joint—this description of joint, notwithstanding its great efficiency, would not often be available in girder work. It might be quite safe to insert rivets in the flanges of the main channel irons, which would yet have a very weakening tendency on the inside wrapper, as the space there for the purpose is diminished by its own thickness. It should be remarked that in the description of joint represented in Figs. 4 and 4*a*, the equation for the value of t_1, the thickness of the inside wrapper, will not hold whenever $l_2 \times l_1 < A$. This is manifest, for at that limit the wrapper is no longer a channel iron, but becomes a thick bar. In small-sized channel irons, the only wrappers which can be applied are an inside and outside piece of bar or plate-iron on the channel part, and the joint then is reduced to that of a simple bar, the flanges being of necessity left uncovered;

nor is the latter alternative of much consequence in the small sections used as struts.

There is another form of iron which may be used for the struts of very large girders, viz: the H section. It is represented in section in Fig. 5*a*, which together with Fig. 5 show the details of a method for jointing two lengths of that form of iron. It consists in applying two pieces of channel iron as inside wrappers, and two pieces of bar iron as the outside ones. Let l equal the length of the side of the H iron, l_1 its breadth measured inside, and t its thickness uniform throughout. Put A = area of the channel iron wrapper, then

Fig. 5*a*.

Fig. 5.



$A=\left(2\left(\frac{l+t}{2}\right)+l_1\right)t$. Make l_2 = length of channel part of the wrapper measured from out to out, l_3 = length of ribs on the inside, and t the uniform thickness, then $A=(l_2+2l_3)t_1$; equating these two values of A we have $(l_2+2l_3)t_1=(l+t+l_1)t$, $l_2=l_1$ and $l_3=\frac{l-t-2t_1}{2}$. Substituting these equivalents in the above equations, we have $t_1{}^2-\frac{t_1(l+l_1-t)}{2}$ $=-\frac{(l+l_1+t)t}{2}$, from which the value of t_1 can be easily obtained. The area of the outside wrappers will $=l\times t$.

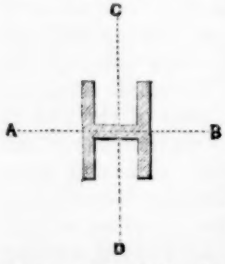
It will be readily seen that this form of iron is not well adapted for struts, either upright or diagonal, when crossed only by single ties, as the riveting at their crossings would have but a very bad hold, in consequence of passing through one side alone of the H section. With double ties crossing it on both sides, it would form a very stiff and efficient web. The above difficulty would also be again encountered in attaching it to the flanges. If it were riveted to the outside of the longitudinal angle-irons, it would only hold by one of its sides. To insert it in between the angle-irons, in the usual manner, would necessitate the placing of them much too far apart; the weight of the packing and distance pieces would also be increased to an unwarrantable amount by this arrangement. It might be attached to the outside of the angle-irons, and the disadvantage of the rivets holding only by one side obviated by forging or flattening down the other side and the middle rib so as to cause the rivets to pass through the whole sectional area. Apart from other objections to riveting the bars of the web of a lattice girder to the outside of the longitudinal angle-irons of the flanges, the serious practical difficulty to be overcome in making good sound workmanship in such a case would more than counterbalance any attendant advantage from its employment. With the exception of rolled iron of a plain bar section and light plate iron, all forging, cutting off, or cranking should be avoided as much as possible in girder work.

The iron should, when in its permanent position in the structure, retain the same shape and dimensions as when it issued from between the rollers. No alteration in these respects can be made, even though performed in the best manner by machinery, without doing more or less damage to the material. In a tension bar the resultant of all the strains may be considered as acting along the centre line or longitudinal axis of the bar; if the original shape of the bar be altered in any manner with respect to this axis, an unequal distribution of metal is produced, and the strength is no longer preserved uniform.

An inspection of Fig. 6, will show the facility with which other forms of iron of equal sectional area, may be substituted for the H form. The line CD, divides the section into two tee-irons of equal area. If the same linear dimensions be adhered to, by doubling the thickness we obtain at once a tee-iron of the required section. In any case calling A the area of the H section, we have, making l the length of the table of the tee-iron, l_1 of the tongue, and t the uniform thickness $(l+l_1)\,t = A$. Again, the figure is divided into two equal channel irons by the line AB, and similarly we find, employing our former notation $(l_2+2l_3)t_1 = A$. Keeping the lineal dimensions as shown in the figure divided by AB and doubling the thickness, will give a single channel iron of the required area as in the instance of the tee-iron. On examining Fig. 6, it will be perceived that the lines AB, CD, divide the H section into four angle-irons of equal area. For one angle-iron of equal area with the figure we should have $(l+l_1-t)\,t = A$; or, what would amount to the same, if t_1 be the uniform thickness of the H iron, $t_1 \times \frac{1}{3} = t$, and the same linear dimensions may be employed as shown in the figure divided by AB, and CD. This form of iron would be found especially efficient as an upright strut or pillar subject to a compressive force alone, care being taken that the strain was thrown well into the pillar, and its bearings firm and well distributed. Its strength would be greatly impaired, from its peculiar form, by any side or diagonal strain brought upon it.

Before concluding our subject for the present, we purpose to make a few remarks representing the net and gross area of a girder—or, what may be used as equivalent terms, but of a more extended range, the net and gross weight; the net weight being understood to mean the actual weight of metal required to sustain the whole load, both live and dead, independent and exclusive of all covering plates, rivets, stiffening, and any other extraneous metal whatever. The gross weight will include the net weight, and all other iron forming part of the girder, but will not include the superstructure or any portion of the roadway. In two similar girders of the same span, the gross weight will bear the same proportion to one another, as the different amount of workmanship, or amount of construction, as it may be termed, bestowed upon each. If in one girder each of the flange plates be half the length of those in the other, there will be double the

Fig. 6.



number of wrappers required, and the gross weight will be proportionately increased. In the Conway Bridge the covers and rivets increase its weight by 31 per cent. For this evil there exist two remedies. One is manifestly the employment of longer plates, so as to diminish the number of wrappers and rivets required for securing the joints: this has been alluded to before and the difficulties to be surmounted mentioned, and it is unnecessary to comment further upon it. The chief obstacle, in so far as the flanges are concerned, is the extra weight or that above what is required by theory, occasioned by the longitudinal riveting of the angle-irons to the plates. Considering the flanges of a girder to consist of two portions, viz: the vertical and horizontal parts, in which the former acts as the connecting medium between the flanges and web, or more properly between the plates and webs, this loss of sectional area and resulting increase of gross weight must always continue until these two parts of the flanges are in one piece. A channel iron would be the simplest form of flange in one piece for the above, but a better section would be that shown below in Fig. 7, in which the only loss of metal would be in the rivet-holes made for the insertion of the rivets which attached the bars of the web to the vertical part of the flanges. Confining our attention to the horizontal part of the flanges, let n = number of rivet-holes punched out per foot run of the girder in the bottom flange, t the thickness of the flange where the holes are made, and w the weight of a cube inch of wrought iron; let x = weight of metal actually required by theory per foot run. Putting d for the diameter of the rivet-hole, the loss of weight equal $ntw\frac{(\pi d^2)}{4}=y$. Accompanying this loss of weight there is also a loss of section, which must be accurately replaced by an increase of metal in the original plate, to keep the weight x constant. Again, as the loss of section or holes for the rivets are filled up by the rivets, their weight must be added, which equals y. The gross weight of the flange per foot run thus becomes equal to $x+y$ instead of x. The increment y is due solely to the necessity of attaching the vertical and horizontal portions of the flanges to one another by continuous longitudinal riveting. To this weight $x+y$ must be added that of the two heads of the rivets, which may be put $=n\times 3d\times w\frac{\pi d^2}{4}=p$.

Fig. 7.

The total gross weight of the horizontal portion of the flange will now equal $x+y+p$. Some idea of the value of p may be gathered from the fact, that in one whole length of tubes in the Britannia Bridge p=229 tons. In bridges designed for carrying a line of railway over ordinary public roads of 20 or 25 feet in width, the flanges might undoubtedly be rolled all in one piece, and also all in one length; and thus the necessity for covers and longitudinal riveting altogether done away with, and the net and gross weight nearly equalized. The lattice or open form of web offers many advantages over the plate or solid-sided girder, with respect to the proportion of gross and net weight. In the

first place there are very few, and in the majority of instances no joints whatever required in the web of a lattice girder. It has already been remarked that no stiffening irons are needed in the web if properly proportioned, and the correct sections of iron employed for the compression and tension bars. The stiffness necessary should be inherent in the weight of metal allowed for taking the strains to be undergone by each bar, and in the peculiar form and shape given to those bars. Contrast this with the fact, that 21 per cent. of the whole weight of the sides of the tubes of the Britannia Bridge is consumed in stiffening irons and covers. It is well known that the principle of the suspension bridge affords the means of supporting a given load with the least amount of material. Independently of the virtue of the principle on which it is founded, one of the reasons is that in these bridges the net and gross weight more closely approximate to one another than in any other wrought iron construction of bridge. It is not to be supposed that all covers, rivets, and the different connexions, can be ever dispensed with where wrought iron is employed, nor do we advocate a parsimonious distribution of material or the employment of a too rigid and consequently false economy, as is very frequently done; but we maintain that a great deal may yet be accomplished in the reduction of the useless and injurious weight of metal too frequently applied in girder bridges, or in other words, in making them less heavy. The Britannia Bridge at the present day has the merit of possessing the largest single span used for railway purposes; and so long as the ordinary wrought iron is employed, much larger spans will not be reached. With the improvements that are in daily progress in everything concerning the manufacture of steel and a superior class of iron, it is more than probable that at no distant period we shall be able to look for a description of iron or steel or their compounds, which will give twice or three times the strength of our common iron, with half its weight and without any increase in price. At present, did the magnitude of the design or other circumstances warrant the expenditure, we might, with an inferior description of steel, which could be obtained by a less expensive process than the usual one, construct bridges for railway purposes, not on the suspension principle, with spans of from 800 to 1000 feet, with ease and security.

MECHANICS, PHYSICS, AND CHEMISTRY.

For the Journal of the Franklin Institute.

Work, Vis Viva, and Momentum. By De Volson Wood.

Prof. of Civ. Eng. Univ. of Mich.

I published an article upon the subject which forms the title of this paper, in the November number of this *Journal* for 1862, since which, I have received several letters which indicate that I failed to make the subject sufficiently lucid.

As the answers to many of the difficulties suggested, possess a gene-

ral interest, and will tend still further to elucidate the subject, I have concluded to consider them in the following article.

The difficulties cannot be in the terms used, for they are merely *conventional*; but rather in a correct conception of what they represent in nature. The real office of each expression is best shown by placing them in contrast, or by showing what they represent when applied to the same moving body. This I endeavored to do in my previous article. I will now try to explain some of the difficulties which have presented themselves to others.

One writer says "Work is performed only in giving increased velocity; not in continuing the motion already acquired." This writer either has a wrong conception of *work* or else he has not fully stated the case. Work is overcoming resistance, and resistance may be constantly overcome at a uniform velocity, or even with a diminishing velocity. It may be, however, that the writer had reference to a body free to move; in which case, the work done upon the body is one of inertia; and it is true that no additional *work* is *stored* in the body unless there be an increase of velocity. In this sense the statement is true, but the language does not restrict it to this.

Again, one says "Vis Viva I take to represent a force." Now this is so vague I am inclined to take issue with it and say that *it does not represent a force; but rather what a force has done, or is capable of doing if it acts for a finite time.* To illustrate, suppose a body is allowed to fall freely under the influence of gravity. Now all will admit that the force of gravity near the earth is constant; but after it has fallen for a time its vis viva is MV^2; where M is constant, and V variable, hence it cannot *represent* a constant force.

Again, suppose a locomotive is attached to a train by means of a spring scale, which indicates a constant force of 300,000 lbs. It moves the train from rest with a constantly increasing velocity, until all the surrounding resistances of air, friction, &c., equal the applied force, when the speed will become uniform.

Now observe that the force is constant and the velocity up to a certain speed is variable, how then can MV^2 *represent the force?* If it be said that it represents the force over and above that necessary to overcome the resistances; then I would observe that the resistance of the air increases with the velocity so that the surplus decreases while MV^2 increases. It does not then *represent* either the force applied or the surplus force.

To show what it does represent, let us suppose, to simplify, the case that all the resistances are constant, and that after the train has acquired a velocity V, the locomotive is removed, and the train is permitted to move on until it comes to rest.

Let S be the space over which it moves,

W the weight of the train,

f the force for a unit of weight, that is the force, as indicated by a spring scale, which is necessary to draw a unit of weight (say one pound) over the resistances, with a low uniform velocity,

$Wf = P$ = the total force, also equal the total resistances.

Then the total work is

$$Wfs = Ps.$$

Now if we conceive P to be applied to the train to move it, and conceive all resistances removed so that the train is *perfectly free* to move, it will produce a velocity V in the train at the end if the space S, *i. e.*, it takes the same space for P to produce a velocity V, without resistance that it does to destroy a velocity V, with a constant resistance of P. The law of motion in the former case is the same as for falling bodies, and in the latter that of ascending bodies.

Then since P is constant $\frac{P}{M}$ is the accelerating force and we have

$$V = \sqrt{2\frac{P}{M}s} = \sqrt{\frac{2Wf}{M}s}$$

$$\therefore \quad MV^2 = 2Ps = 2Wfs.$$

Now observing that P is the true value of the force, we see the second member is twice the force repeated S times, in other words it is twice the sum of all the forces exerted on each unit of the space, S—and the first member must *represent* the same thing.

If the resistances are variable, this becomes general by considering the force on each element of space, and adding them all together, which gives rise to the well known expression $\int P ds$.

Another correspondent says, "A sliding body has a certain vis viva; now if we double the velocity the vis viva will be fourfold; and unless we conclude that dynamical friction increases with the velocity, I do not see how these factors preserve their equality." Dynamical friction does not increase with the velocity; hence we must reconcile the facts on other grounds.

Let φ = the total amount of friction.

$\therefore$ φs = the work on space S,

φs^1 = " " s^1,

V = the velocity at the end of space S,

i = angle of inclination of the plane.

The motion of the body follows the law of falling bodies;

$$\therefore \quad s = \frac{V^2}{2g.i.}$$

and for double the velocity

$$s^1 = \frac{(2V)^2}{2g.i.} = \frac{4V^2}{2g.i.}$$

$$s^1 = 4s, \text{ and } \varphi s^1 = 4\varphi s.$$

So that by doubling the velocity we quadruple the *stored work*. We may observe that the body must pass over four times the space to produce twice the velocity.

Another says, "Gravity does not add equal increments of space in each element of time, and hence doubts whether Wh really expresses the work which gravity does upon a falling body." He admits "that it adds equal increments of velocity in each element of time."

His statements in regard to gravity are correct. Let us apply the latter and see where it will lead us. If a resistance P, be moved with a uniform velocity V, then would the work done in a unit of time be

PV and in a time t, it would be $PVT = PS$ . . . (1)

But in a falling body the resistance overcome is the weight, and the velocity is uniformly increasing; so that the work done during any element of time, Δt, is by (1)

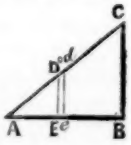
$$W V \Delta t \quad . \quad . \quad . \quad . \quad (2)$$

But from a law of falling bodies,

$$V = gt \text{ or } \Delta V = g\Delta t,$$

which in (2) gives $\frac{W}{g} V.\Delta V = MV.\Delta V.$

This solved by the calculus gives $\int M V \, dV = \frac{1}{2} MV^2$, but as I wish to make it intelligible to those who do not understand the calculus, I will give the following way of finding the sum of all the products of V.ΔV between 0 and V. Let any ordinate, as DE, represent the velocity, V. Since the velocity uniformly increases, the line AC will be the limit of the ordinates. Let $Ee = \Delta V$, then will the vanishing trapezoid be ultimately equal the rectangle of which DE is the base and Ee the altitude; hence $DEed = V\Delta V$. The sum of all the infinitesimal rectangles will equal the area ABC. The sum of all the altitudes of the rectangles becomes $AB = V$; hence the area equals $\frac{1}{2} V^2$ and the expression becomes $\frac{1}{2} MV^2$.



My attention has been called to several other points, but they indicate too limited knowledge of the subject, or too little thought bestowed upon it to make a reply to them profitable in this connexion.

Momentum has no reference to the *resistance* to be overcome, but to the *motion* which a body of known mass and velocity will induce in another mass free to move, when the former impinges upon the latter. As the expression indicates, it pertains only to the relations between masses and velocities of different bodies. Momentum may be considered as the measure of an impulsive force; one-half the vis viva, the *sum* of all the energies (or forces) exerted to cause a velocity V. I trust that the relation between momentum and work, in the example used in my previous article, is sufficiently explicit. I will, therefore, drop the subject until circumstances induce a further consideration of it.

For the Journal of the Franklin Institute.

The Forces; Slave Labor; Working Land by Steam; A Cycloidal Cultivator.

Of mundane agencies FORCE—physical energy or power—is unique and supreme. All that we see, and feel, and hear, and do; light and life, forms, qualities, and motions, proceed from it. Its proper use comprises the whole economy of life. It is emphatically *the* talent given to men to profit withal. It places the destiny of every people in their own hands, for the amount at their disposal is not limited, nor are the

means of employing it. If it had nothing to do in originating differences between races, it is an imperishable criterion of their varied conditions. While the foremost keep startling the world with wonders wrought by it, others sleep on unconscious of its value. A generic term, it consists of various forces, animate and inanimate; and as the more the latter are brought into play, the less become demands on the former, hence they who add to our working stock of insensible energies, and they who extend their applications to great industrial interests, are, or ought to be, classed among public benefactors. In cheapening force (by increasing its amount) they cheapen its products, and by lessening human drudgery, they help to secure for their species, as respects food and raiment, that condition which is essential to mental and moral culture.

The forces constitute a series that grows with man's growth. They lift him from one stage to another. Taking him up on the lowest and bearing him along towards the highest. We know what the former is, but of the latter we catch only glimpses. With his own strength only man is, and has always been, a savage. The ass and the horse raised him a degree, the ox and the buffalo another. Yet from our standpoint how low appears the status of the Arabs, the Tartars, and others, and with the additional elephant and camel, that of the people of India and of Asia generally. In running and falling water, and subsequently in wind, forces very different from the former were realized. Inanimate and therefore insensible to hard usage, and requiring neither food nor rest, they added vastly to his working capital. What the result was, we have a graphic representation in the oldest of existing people—the Chinese. They, of course, are considered as having acquired only a partial civilization, but we apprehend that it is as complete as those forces could effect. With the same, no people have surpassed them. We doubt if any have equaled them. The next movement was the greatest, and is recent. The agent, as may be supposed, was not, like the foregoing, derived from bodies put in motion and kept moving by nature. The advance of modern society could not have been effected by one, nor by all of them: they prepared the way for the leap, but could not take it. It was the result of the advent of a higher class of agents; such as demand a further stretch of intellect to evolve and control—such as man himself is to call forth for himself from bodies at rest.

Competent (as far as reason can reach) to meet the expanding wants of society for all time, they may be the last as they assuredly are the greatest in the series. Varied and palpable proofs we have of national prosperity and power in the augmentation of their leading representative, *Steam*. Look abroad and see if the first nations are not its greatest consumers. The richest they must needs be, since wealth arises solely from productive labor, and they the chief dealers in it. Close the coal mines of Great Britain, or let the fires of her steam motors die out, and she will descend at once from her high position among the nations. No possible amount of human ingenuity and industry and of animal power, could arrest her declension. No, not the living

twelve hundred millions of human beings could do the work which steam is doing for her. And what it is doing is but a fraction of what it is destined to do for her, and for all who choose to employ it. There can be no monopoly of it, no quarreling about it. Free as air, cheap as wood and water, those who have it not have themselves to blame.

[*En passant:* Had the study of nature's laws respecting the Forces she has provided for man, by which to work out his destiny, been included, as it ought to be, in College instruction, this terrible war that is converting whole States into graveyards, might have been postponed—perhaps had not broken out. At all events, if its authors and abettors had been impressed with the fact that there are forces ten times less costly than negro labor, and ten thousand times more abundant and effective—that one of them can do more work in a week than their slave population in a year; that, in short, inanimate powers are ordained to supersede human slavery, and even to ameliorate the labor of animals, they surely would have paused ere calling up the demons of rapine and blood—they would have been reminded that the vast amounts of slave labor already displaced by steam are an earnest of more, and possibly have been disposed to wait for further developments of a power that seems all but omnipotent for human affairs, and only waiting to be applied to them. It is true that great social elements make themselves felt slowly. They must do so if genuine, for nature produces nothing abruptly; but steam has been growing upon us for a century or more, and has now become widely acknowledged as prominent among productive forces.

If there is anything made manifest in the economy of the planet, it is that living forces are weak, irregular, and quickly consumed—that they are introductory to the powerful, untiring, and exhaustless inorganic energies—that it is by these the work of the world is to be done, and that compared to them the amount of animal labor is a trifle, and that of human toil—a nothing. Slavery, irrespective of the habits it imparts, is purely a question of labor. When it becomes less profitable than other forces at hand, that is the signal for its dismissal. Its upholders may object, but it must yield to the law, and give place to inanimate power: the world is moving and is not to be stayed by them or for them. Hence, secessionists have undertaken something like an impossible problem in attempting, at this day, to raise a new political structure with negro bondage for its basis and corner stone—and this too in immediate contact and competition with free white institutions. The time for it has passed. The world has outgrown it. But such is not their belief. Reared in an atmosphere of slavery, they do not perceive why other people should object to live and move and have their being in it. There have been old hospital patients that preferred the tainted air of the wards to fresh breezes outside.

It is one of the thousand proofs of the support enlightened Christianity receives from the doctrines of science—every one of which *is* divine—that the Creator does not appear in His works in the character of a Moloch on whose shrines negroes are to be perpetually offered up, but

in that of a tender and impartial parent, who has made the amplest provision for relieving every race from oppressive labor.

It is vain to wish the South had been willing to resolve the difficulty into a simple question of labor. Perhaps it is equally futile to wish the North would offer a premium for a practical demonstration that slave labor can be replaced by other agents with advantage to the planters. The offer of a single day's cost of the war would be more than sufficient to set the keenest of the world's engineers at work to extend the application of steam to out and in door labor, and to stir up the leading chemists in every country to hasten the birth of forces awaiting their assistance—among them *atmospheric pressure*, the use of which as a popular motor, and the most popular one, seems only held back as by a thread. A cheap device by which to decompose or otherwise promptly to get rid of air or vapor under a piston is all that is wanted. That gained, slavery could not survive it an hour—and it is one of those things that may be hourly looked for.]

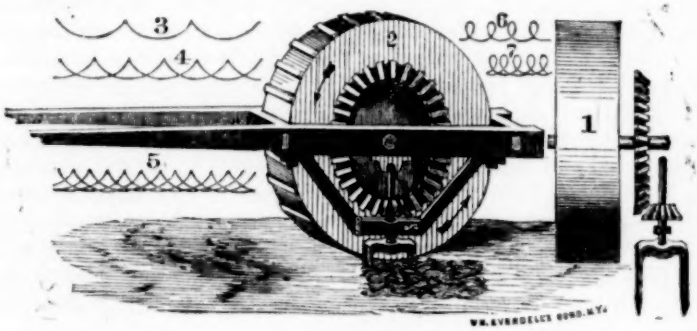
The superiority of modern arts is due to the introduction of steam power. It has diminished the cost by increasing the products of industry—it has added to the elegances and multiplied the conveniences and comforts of life, wrought a revolution in almost every department of manufactures, and established a system of traveling by sea and land that would have been deemed miraculous a century ago. It has sown knowledge broadcast by making books common as waste paper. Yet on one great division of labor it has scarcely entered. It has done nothing, or next to nothing, in tilling the land. Not a tithe of it has anywhere been brought under culture. True, an adequate force was long wanting, but one is now sufficiently developed, nor is there room to doubt that steam is ordained to rival in the field what it has accomplished in the factory: that is, to double and quadruple the earth's harvests—to provide, and keep providing, breadstuffs for a population destined, through cycles of ages, to increase in numbers and intelligence. Why has it not been pressed into this service? Are the arts not sufficiently matured properly to apply it? Or is not the delay rather due to attempts to work an old implement with it—one to which it cannot be profitably adapted?

The plough arose from attempts to substitute animal for human labor, and certainly a device better adapted for the purpose, in the simplicity of its form and action, and in the direct application of the power, it would be difficult if not impossible to name. It belongs however to living forces. It came in with them and cannot long survive them. It has been the subject of numerous modifications without affecting its cardinal features, and these it must retain as long as it is associated with draught animals.

We all know that as the arts mature, modes of employing Force become more and more varied. To the extensive adoption of circular for straight motions may be ascribed, in no small degree, the progress they have made in our times; and no one needs reminding that the general effect of steam on transmitting and operating mechanisms has been the substitution of continuous-rotary for alternating-rectilinear

movements—a change to which the plough cannot conform without itself being changed. Effectually to cultivate the soil by steam, the working implements must, we apprehend, be revolving ones, and their action, instead of being derived from the forward movement, be independent of it.

Suppose a modern engineer who had never turned his attention to agriculture was employed to bring extensive tracts of wild land into cultivation—to break up the soil and comminute it to the depth of 8 or 12 inches, how would he thus prepare it for seed? I venture to say that he would no more propose any thing analogous to the plough than he would reinvent the flail for thrashing, wait for a wind to separate chaff from wheat, or pound rice into flour in a mortar. Steam would be his power, and to it the form and action of his implements would have to accord. Their movements would be rotary. They would pare away the soil to the requisite depth before them, and leave it thoroughly lightened up behind them. This they would effect, instead of a partial disintegration by dragging a blunt instrument horizontally through it. In brief, I suppose he would develop something more or less allied to the following device.—Though designed to be worked by steam, it is figured as adapted to horse power, on which account the shafts and large drum are introduced—appendages useless when steam is used.



It consists simply of two perpendicular prongs with flattened or sharpened edges—or strong narrow knife blades—formed on the lower end of a short vertical axle, with a pinion which, by its connexion with a bevel wheel, rapidly revolves them. See fig. 1. One side of the bearing piece that supports them is hinged and fastened by a hasp, to allow them to be readily slipped into and out of their place. See fig. 2. Such is the implement, and the whole of it. All the rest is merely to work it. It is little else than a revolving edged fork. The length of the prongs or cutters is of course determined by the depth of soil they are to stir up. As their cost is trifling, three might be kept on hand—a 4-inch, an 8-inch, and a 12-inch one.

In some cases one, but in almost all cases two prongs will be sufficient. If three are required they should be arranged triagularly—that is, at equal distances from the axle. By no possibility can one run

into the track of another. The ordinary tracks of a single prong are represented at 3 and 6, of two at 4 and 7, and of three at 5.

The tracks are cycloidal curves, which may be varied from undulating or wave lines to intersecting circles, or figures approaching circles.

The action of the ploughshare is well known to be that of a wedge pressing on the soil beneath it, and with a force equal to that which displaces what is above it.—Every year's ploughing adds to the compression, since every force brought to act on the surface reacts on the undisturbed base. The effect is obvious in old fields. Gravelly and sandy bottoms have become hard and compact almost as stone, and subsoils so dense as to exclude the circulation of air through them. With this cultivator nothing of the kind can occur, since the acting parts transmit not a particle of pressure below. Moreover, neither the feet of the ox or horse, nor the wheels of a steam motor, travel over the ploughed ground. Every thing moves over the unploughed surface except the prongs, and they are suspended from above.

As there is no dragging action there can be no slipping of the wheels of a steam motor—that which has given experimenters most trouble—for the force given to the prongs reacts *within* the carriage—not without. Hence the forward force has only to move them up to their work—not to do it. Their action may be likened to the teeth of circular saws cutting their own way through a log, and only requiring it to be kept up to them. As the prongs act equally in any direction, their motion has only to be reversed at the end of a furrow or field.

I may be wrong, but I suppose any required depth of fair soil may be as completely loosened, lightened up, and prepared for seed, *at one operation*, by this cultivator, as by any one now in use, and with a smaller outlay of force. That which is required to cut 12 inches deep can, I think, be little more than half what is consumed by subsoil ploughs. It is known that thin soils, resting on hard pan, yield greater crops if the pan be loosened to permit the roots to shoot into it. Now these revolving prongs can readily work into it, and without mingling it with the soil, while a succession of roots will gradually fertilize and enrich it.

With the exceptions of breaking up new lands, and working among stones and roots, there seems to be few operations of the old plough which this cultivator cannot perform. It appears to be well adapted to give an intimate and perfect stirring up to light and medium soils; and such is said to be the general character of those of England and France, and of other parts of Europe—also of India, and the sugar and cotton lands of our Southern States.

The hold the cutting prongs take, can, of course, be regulated to slice off portions of any determined thickness—and as they can be arranged that even a man's power could work them in fine soils, possibly market gardeners may find an implement of the kind more economical than their ordinary apparatus.

After citing supposed points in this Cultivator, it would be just to indicate its weak ones. But what they are, practical men must deter-

mine. It is not supposed to be free from defects, nor do I think, as already intimated, that it is calculated, in its present form, to supersede the plough, except in fine, light, and loose medium soils. Still, where it cannot act the part of a principal, it may serve as a useful accessory. The question turns on the value of this *cycloidal movement of vertical blades or cutting prongs.* The principle is a novel one in agriculture, and may, at least, suggest an improvement in working the soil, if it be not itself one. E.

New York, Dec. 21, 1863.

On a new Method of Casting Guns Hollow. By Mr. OUBRIDGE.

From Newton's London Journal of Arts, December, 1863.

The author remarked that the subject which he proposed to consider that evening was one upon which much had been already said and written. He believed, nevertheless, that it was not exhausted; for it was a fact that, as yet, no heavy gun had been produced which answered all requirements. Brass guns had had their day, cast iron guns had been long used, were at one period almost totally condemned, but were now cropping up again, and much might be adduced in their favor. Wrought iron, in multifarious forms, had been employed in the manufacture of guns, each form having peculiar advantages; but the results, on the whole, were not satisfactory. Steel had also been introduced for the purpose; but in spite of its great cohesive strength, it had been found impossible hitherto to make from it good, sound, and serviceable guns of large calibre. Many practical difficulties stood in the way of its employment in this direction, although certainly some very successful efforts had been made to construct light and small pieces of ordnance of that material. Compound guns, composed of cast and wrought iron—the one encasing the other—had also been tried, with variable and uncertain effects; but as yet no absolute rule had been deduced for the guidance of those whose duty it was to manufacture heavy guns. Time, and the expenditure of much more of the public money, might effect this great desideratum; but it had not yet been achieved; and it was, therefore, the duty of practical and scientific men to endeavor to solve the problem "how best to manufacture heavy guns?" He (Mr. Oubridge) intended to contribute a few items to the mass of existing information on the subject; and he might state that such knowledge as he had to impart had been gained from his own experience and experiments in the iron foundry. It was required, in the production of large pieces of ordnance, that the material used should be made to offer the fullest possible resistance to the bursting strain to which it would eventually be exposed, and that the cohesive strength of that material should be completely maintained. Perhaps, before advancing his own views, he might be permitted to refer to the method of casting heavy guns, which had been largely practised during the unhappy contest which still raged in America. The name of Dahlgren would no doubt be familiar to his hearers in connexion with the American civil war, and the guns which

were known as "Dahlgrens" possessed some distinguishing features. They were cast hollow, the theory of their inventor being that it was desirable "to reduce the neutral axes of his gun as nearly as possible to the centre of the thickness of the metal of which it was formed." This would be clearly seen to be the case from the diagram produced. If, on the contrary, the gun were cast solid, the cohesive power of the metal would be diminished as it approached the centre. By casting the gun hollow, this deteriorating process would be lessened by the pressure of the core in the mould. Dahlgren had evidently well considered the laws of cohesion and disintegration which governed these results. He had also adopted the plan of assisting the cooling and contracting, by pouring a stream of cold air through a tube of iron inserted in the core barrel. In this he was perfectly right, for it ought to be understood that if the cooling process operated entirely from the outer portion of the casting, it was, to use a familiar illustration, like casting an iron ring upon a mandril—an operation which they all knew would be a senseless proceeding. This plan had, however, been persistently followed until the Armstrong gun was introduced. He (Mr. Oubridge) had, several years before, submitted to the Select Committee of the Board of Ordnance, a scheme for casting heavy guns hollow, but the reply of that body was, as usual in such cases, unsatisfactory. At the same period an American gentleman made some attempts to accomplish, in this country, the same thing, but without success. For his purpose six cupolas were erected near the Charlton Pier, on this side of the town of Woolwich, and the experiments there conducted were of a costly character. The theory was not the less a true one; the effect lay in its imperfect realization.

There was no doubt in his own mind that homogeneity would be obtained to a far higher degree in large iron castings if they were made hollow in place of being solid. He had had an opportunity a few months back, indeed, of practically demonstrating the fact. A large hydraulic cylinder, requiring 17 tons of metal to cast it, had to be produced. It was to have a very small hole through the bottom end, where the iron would be 15 inches thick. He saw that the intense heat would inevitably melt the core barrel long before the metal composing the cylinder ceased to be fluid. He therefore assumed it to be a favorable opportunity for putting to the test his cherished theory. The casting was to be produced in the foundry of the Messrs. Simpson, at Pimlico, and Mr. Thompson, the manager for that firm, giving his consent, he determined to cool the core by means of an internal current of cold air. The plan of operation was very simple. A perforated tube was inserted in the core barrel, and then communicated, by means of a pipe and valve, with the blast. As soon as the mould was filled with metal, the valve was opened, and the cold air forced into the tube found an outlet through the perforations, whence it impinged upon the barrel: this latter was also perforated so as to allow of the escape of gases from the interior of the core. The result was a complete success, and the cylinder was as sound and homogeneous as could be desired. The cooling process had thus gone on from the centre, instead of the

exterior of the casting. When the operation had, as he considered, lasted sufficiently long to prevent the metal remaining in a fluid state, he caused the blast valve to be shut, and in half an hour the core barrel was found to have melted in one place, leaving an aperture three inches in diameter. The blast was then again turned on, and in less than ten minutes the barrel became black. By this method, which was of the most easy application, it was possible to reduce the temperature of the interior of a casting to almost any extent, and in a very short space of time. He desired to throw his mite into the treasury of human knowledge. It was for others to put it out, if they chose, to interest. Perhaps those who might be called upon to produce heavy castings, whether for the purposes of war, or in fulfilment of the more hallowed behests of peace, would be induced to take up the hints which he had that night ventured to give. It was to be hoped that the practical founders of England, especially, would be permitted to exercise their own judgment as to the selection of the various irons best suited for partial castings, and that in the manner of producing those castings they would not be hampered by mere theorizing or "paper engineers," of whom too many had crept into the noble profession of which he was a humble member. He had a reverence for art and science, and for its practical exemplars, but not even ordinary respect for those who pretended to be their disciples, but were nothing more than pretenders.

After the reading of Mr. Oubridge's paper, which was well illustrated by diagrams, a discussion ensued, and this was followed by a vote of thanks.

Proc. Asso. Foremen Engineers.

Decision of the Board of Examiners in Chief in the matter of the Application of J. S. MASON, *for Letters Patent for Design for a Label.*

Reported by H. HOWSON, ESQ.

U. S. Patent Office, October 16*th*, 1863.

Application (A) of James S. Mason for a Patent for a Design for a Label.

On Appeal to the Examiners in Chief.

To understand the merits of this appeal requires a brief explanation of the design.

It is intended for a label to be placed on boxes of shoe-blacking, and is based upon the well-known representation of a boot so highly polished as to reflect images of the objects around it. Starting with this conception, the artist has depicted a boy holding a boot out to a dog, which is startled by the sight of his own figure in it, while the boot-black in the rear is dancing about in ecstasy at the effect. Simple as this is, two important questions grow out of the application.

In the first place, the applicant has described in his specification, and portrayed in his drawing, a duplicate design resembling the other, except that a fighting cock, irritated at the reflection of his form in the boot, is substituted for the dog. And he claims a patent which shall protect both the original design, and this modification.

His argument is, that the merit of the artist lies not in the idea of representing a boot shining like a mirror, nor in the representation of its effects upon the dog or cock; but in what would usually be termed the subordinate figures in the scene, the boy holding the boot out to produce the effect, and the boot-black delighted with it. There is force in the argument; but admitting its soundness, it does not follow that he may monopolize, not only the delineation of the figures which he regards as really the principal features originated by him, but also several different arrangements of figures, with which he may choose to fill up the scene. If there is no merit in these, or if the applicant does not wish to patent them, he may confine himself in his claim to the other more important parts of the representation.

Or he may, perhaps, claim them in one class, and claim them in combination with one set of figures to fill up the sketch in another. But he cannot claim two sets of figures, and thereby monopolize under one patent, what may be two entirely different designs, each perhaps highly artistic and valuable in itself. The Examiner was correct therefore in rejecting the application as long as the petitioner insisted on retaining both modifications of his design in his specification, and so framing his claim as to cover both.

The applicant proposed to amend his specification by inserting in the claiming clause the words "substantially as shown in the drawings;" so as to protect himself from imitations with slight and unimportant variations. This the Examiner refused to allow, and insisted that "a design admits of no variation; any alteration gives matter for special consideration."

There can be no doubt, however, that a mere evasive variation in the design, would not prevent a counterfeit from being furnished as an infringement. The same principle of law would apply in such a case as in actions upon patents for machines. In both instances the patent will be construed exactly as if the phrase "substantially as described" was in the claiming paragraph. This was decided in the most explicit terms as to patents for designs by the late Judge M'Lean, in Root *vs.* Ball and Davis, 4 M'Lean's R. 177; and, until that case is overruled, the question cannot be considered with propriety, as open to discussion elsewhere. And, since the patent is so construed in one case, as well as in the other, there can be no objection to inserting the corresponding expressions in patents of both kinds, and making them declare what they have been adjudged to mean. The Examiner ought, therefore, to have allowed the amendment.

It must not be understood, however, that such an amendment will have the effect of protecting the applicant's right to both the figures he has described.

That cannot be done by one patent, as we apprehend; though the same things may perhaps be effected, substantially, by patenting the remaining parts of the design. It is for the applicant to determine whether he will adopt that course. But while we consider that he has the right to use the expression, "substantially as described," in order to give the fullest notice of what are really his rights, we hold

that he has not the right to embrace both his complete designs under one patent; and that he was, therefore, rightfully rejected for refusing to confine his claim to one of them.

The specification contains a second claim embracing the printed inscription around, and underneath the pictorial design. The applicant has therefore attempted to obtain a patent for a design embracing nothing but printing with ordinary type, and the decision of the primary Examiner refusing a patent, was affirmed by this Board, the 8th May last. The reason for their judgment in that case apply with equal force to the claim before us, and it must be regarded as having been properly denied.

The decision of the primary Examiner is, therefore affirmed on both points.

S. H. Hodge,
J. J. Coombs, } *Examiners in Chief.*

On the Direct Correlation of Mechanical and Chemical Forces.

By Henry Clifton Sorby, F.R.S.

From the Proceedings of the Royal Society, No. 56.

Perhaps it may be thought somewhat strange that a geologist should undertake such a subject as the correlation of forces; but the very fact of my being a geologist has led to the investigation of which I now purpose to give a short preliminary account. In studying general chemical and physical geology, and especially in examining the microscopical structure of rocks, I have for a number of years been greatly perplexed with a class of facts which pointed both to a mechanical and to a chemical origin. At first I attributed them either to a mechanical or a chemical action, or to the two combined; but in most cases no satisfactory explanation could be given. At length, however, facts turned up which altogether precluded any supposition not involving direct correlation; for they most clearly indicated that mechanical force had been resolved into chemical action in the same way as, under other circumstances, it may be resolved into heat, electricity, or any other modification of force, as so ably described by Grove in his work "On the Correlation of Physical Forces."

The effect of pressure on the solubility of salts has already been made the subject of speculation and experiment,* and a considerable number of facts have been described, showing that pressure will more or less influence such chemical actions as are accompanied by an evolution of gas, so that it may cause a compound to be permanent which otherwise would be decomposed;† but the results were for the most part so indefinite and unconnected, or of such a character, that

* Perkins, Ann. de Chim. et de Phys. vol. xxiii. p. 410. Sartorius von Waltershausen, Gottingen Studien, 1857. Bunsen, Ann. der Chem. und Pharm. 1848, vol. lxv. p. 70. Favre, Comptes Rendus, vol. li. p. 1027 Thomson, Proc. Roy. Soc. vol. xi. p. 473 (1861).

† Sir James Hall, Trans. Roy. Soc. Edinb. 1812, vol. vi. p. 71. Wohler, Ann. der Chem. und Pharm. vol. xxxiii. p. 125. Babinet, Ann. de Chim. et de Phys. (2) vol. xxxvii. p. 183. Lothar Meyer, Pogg. Ann. vol. civ. p. 189. Beketoff, Comptes Rendus, vol. xlviii. p. 442. Gassiot, Brit. Assoc. Report, 1854, p. 39. Favre, Comptes Rendus, vol. li. p. 1027. Berthelot et Pean de Saint-Gilles, L'Institut, 1862, p. 257. Gmelin's Handbook of Chemistry, published by the Cavendish Society, vol. ii. p. 293.

Mr. Grove does not allude to the *direct* production of chemical action from mechanical force. That this is, however, extremely probable will be evident to all who have considered the manner in which the various physical forces are correlated; for if mechanical force can be produced by chemical action, why should not the converse be true? In this paper I shall endeavor to show that such is really the fact, and that in some cases the mechanical equivalent of the chemical force may be determined.

In order to obtain the necessary great pressure, I have made use of a modification of the method employed by Bunsen; but instead of filling the tubes at the ordinary temperature of the atmosphere and then gently heating them for several hours, I, in the first instance, filled them at a temperature of 10° or 20° C. lower, so that when finally sealed up they contained considerably more liquid than they could hold without pressure at the ordinary temperature of the atmosphere at the time being; and thus, by its tendency to expand, this liquid and any thing enclosed in the tube were subjected to a very great pressure. By keeping the tubes in various parts of the house, according as the weather varied, I have been able to maintain for several weeks or even months a pressure of, for instance, about 100 atmospheres, as measured by means of a capillary-tube pressure-gauge enclosed within the larger tube. Since in all cases I had a second tube which from first to last was treated precisely like the other, pressure excepted, I have been able to determine the effect produced by the pressure with very considerable accuracy—at all events so as to leave no doubt whatever about the general facts. At the same time I wish it to be understood that the results described below must be looked upon only as approximations to the truth.

I will first call attention to the well-known influence of pressure on the fusing-point of various substances, since it is a connecting link between well-established facts and those I am about to describe. Bunsen* and Hopkins† have shown that substances which expand when fused have their point of fusion raised by mechanical pressure; that is to say, since mechanical force must be overcome in melting, the tendency to melt must be increased by heat before that opposition can be overcome; and the pressure required to keep them solid at any temperature above their natural point of fusion may be looked upon as the mechanical presentative of the force with which they tend to fuse at that temperature. Professor W. Thomson‡ has shown that, on the contrary, water, which expands in freezing, has its point of fusion lowered by pressure; that is to say, since mechanical force must be overcome in crystallizing, crystallization will not take place under increased pressure unless the force of crystalline polarity be increased by reducing the temperature. Thus, calculating from his experiments and from the known latent heat of ice, and assuming that no heat is gained or lost by contact with external objects, if we had 1 part of ice and 100 of water at 0° C., and then applied a

* Pogg. Ann. 1850, vol. lxxxi. p. 562.
† British Association Report, 1854, p. 57.
‡ Trans. Roy. Soc. Edinb. vol. xvi. p. 575.

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pressure of 103 atmospheres, the ice would, as it were, dissolve in the water, the whole would become liquid, and the temperature be reduced to —·792° C.; or, in other terms, at that temperature the tendency to crystallize is exactly counterbalanced by that pressure.

Now I find that similar principles held true with respect to the solubility of salts in water. If, when they dissolve, the total bulk increases, pressure reduces their solubility; whereas if the bulk decreases, pressure makes them more soluble; in other words, solution or crystallization is impeded by pressure according as mechanical force must be overcome in dissolving or in crystallizing.

Various authors have written on the volume with which salts enter into solution;* but since the subject before us requires a different class of facts to be taken into account, I shall base my conclusions on my own experiments. The volume with which salts exist when in solution, assuming that of the water to remain unchanged, varies greatly in the case of different salts, and also according to the amount in solution and the temperature. Thus, taking sal-ammoniac as an example, when there are 3 per cent. in solution in the volume is as if it expanded 3·40 per cent. on dissolving; whereas when 25·55 per cent. are in solution, the expansion is 11·36 per cent.; and when nearly concentrated at about 13° C., an additional quantity expands on dissolving 15·78 per cent. In by far the greater number of cases, however, there is a contraction on dissolving, and the amount gradually diminishes for each additional quantity entering into solution, so that the mean result is very different from what occurs when the solution is dilute or nearly saturated. It is this contraction or expansion when a small additional quantity is dissolved in a nearly concentrated solution that must be taken into account in the following calculations.

In determining the influence of pressure on the solution of salts, I found it requisite to adopt somewhat different methods according to the peculiarities of the salts. In some cases I sealed up in a saturated solution portions of the salt in clean, solid crystals, and determined the effect due to pressure from their loss in weight; whereas in other cases I sealed up solutions containing more salt than could be dissolved at the temperature at which the experiments were made, and determined the effect of pressure from the difference in the weight of the crystals deposited; being of course careful to make allowance for any difference in the amount of solution in the tube with pressure and in that without, and to avoid any error that might be produced by a different temperature. In all cases I have had a tube with pressure and another without, treated from first to last in precisely the same manner, and kept exactly the same temperature, so that pressure was the only difference; and usually the effect was so well marked that there was no doubt about the result. In the case of chloride of sodium, solution goes on so slowly, and the mechanical

* Playfair, Chem. Soc. Quar. Journ. vol. i. p. 139. Michel and Krafft, Ann. de Chim. 2 ser. vol. xli. p. 471. Schiff, Ann. der Chemie, vol. cix. p. 325; vol. cxi. p. 68; vol. cxiii. p. 349. Gerlach, Specifische Gewichte der Salzlosungen, &c., 1859. Tissier, L'Institut, 1859, p. 158; 1860, p. 281. Kremers, Pogg. Ann. vol. lxxxv. pp. 37 and 246; vol. xciv. p. 87; vol. xcv. p. 110; vol. xcvi. p. 39; vol. cv. p. 360; vol. cviii. p. 115. Billet, These de Chimie.

equivalent of the force of crystallization is so great, that if pressure had been applied for only a few hours one might have concluded, with Bunsen, that pressure has no influence on solubility; but, by maintaining it for a week or more, there was no difficulty whatever in perceiving that a solution which was quite saturated without pressure, dissolved more under a pressure of about 100 atmospheres.

The solubility of a salt in water appears to me to result from a kind of affinity which decreases in force as the amount of salt in solution increases. This affinity is opposed by the crystalline polarity of the salt; and when the two forces are equal, the solution is exactly saturated. As is well known, a change in temperature alters this equilibrium; and, according to my experiments, mechanical pressure relatively increases one or other of these opposing forces, according to the mechanical relations of the salt in dissolving. At all events in the case of chloride of sodium the extra quantity dissolved under pressure varies directly with it for such pressures as glass tubes will resist, in the same manner as, according to Thomson's experiments, the fusing-point of ice is reduced. Thus I found that for a pressure of 49½ atmospheres the extra solubility was ·176 per cent., and for 121 atmospheres ·431, which are almost exactly in the same ratio. Hence, if s be the amount soluble without pressure, under a pressure of p atmospheres, the solubility at the same temperature would be $s + ps$, where the values of s and s are independent, and vary for different temperatures and different salts. Future experiments may perhaps show that this conclusion should be modified; but yet it will be well to adopt it provisionally, in order to compare together the mechanical relations of different salts which otherwise would not be so intelligible.

According to Michel and Krafft* and to Schiff,† sal-ammoniac is the only salt known for certain to occupy more space in solution than when crystallized. Hence, under pressure mechanical force must be overcome in dissolving, and experiment shows that, on this account, the relative force of crystalline polarity is increased and the solubility decreased. This is the reverse of what results from an elevation of the temperature, so that the effect cannot be due to heat generated by the pressure, but must be the direct consequent of pressure. Calculating from an experiment where the pressure was 164 atmospheres, which gave a decreased solubility of 1·045 per cent. of the whole salt in solution, a pressure of 100 atmospheres would cause ·637 per cent. less to be dissolved than is soluble at 20° C. without pressure, and the pressure requisite to reduce the solubility to the extent of 1 per cent. would be 157 atmospheres. Expressing this fact in other words, we may say that a pressure of 157 atmospheres is the mechanical force with which the salt tends to dissolve in a solution containing 1 per cent. less than can dissolve at the same temperature without pressure, because the two forces exactly counterbalance one another. In a still more dilute solution the force would of course be still greater, in ac-

* Ann. de Chim. 3 ser. vol. xli. p. 471.
† Ann. der Chemie, vol. cix. p. 325; vol. cxiii. p. 329.

cordance with the fact of a greater pressure being necessary to prevent the salt from being dissolved. Supposing then that we had a solution a trifle more dilute than that just named, and in such indefinitely large quantity that a cubic inch of the salt could dissolve in it and yet produce no sensible change in its strength, so that from first to last it might be considered to dissolve under a pressure of 157 atmospheres, and also supposing that it was rigidly enclosed on all sides but one, so that the whole expansion must take place in one direction over an area of one square inch, since on dissolving there is an increase in bulk from 100 to 115·78, the solution of this cubic inch would, as it were, raise 2355 lbs. through the space of ·1578 inch. This is mechanically the same as 371½ lbs. raised 1 foot, or, the specific gravity of the salt being 1·53, the same as 171 times the weight of the salt itself raised 1 metre. Since it involves no arbitrary unite but the metre, I shall adopt the last expression as the measure of the total amount of mechanical work done by the solution of salts which expand in dissolving, and which may conversely be looked upon as the measure of the mechanical force rendered latent and, as it were, expanded in the act of crystallization when crystals are deposited. The value of this mechanical equivalent of course varies with the strength of the solution, as already remarked.

In the case of salts which occupy less space when dissolved than when solid, pressure, like the increased temperature, causes them to be more soluble; mechanical force is lost when they dissolve, and is, as it were, expended in giving rise to solution. When water thus containing more of a salt than could otherwise be dissolved at the same temperature is just saturated under any given pressure, the amount of pressure represents the force of crystalline polarity tending to cause the salt to be deposited in a crystalline form, but which is exactly counterbalanced by that pressure. I will not give the details for each salt, but subjoin a Table of the results at which I have arrived for such as illustrate particular points of interest, the calculations being all made in accordance with the principles already described. I also give them in the case of water, calculated from Thomson's experiments, assuming that, when ice meets and mixes with water, it may be looked upon as dissolving in it; and, as will be seen, the mechanical force thus deduced is of the same general order of magnitude as that generated by the crystallization of salts.

	I.	II.	III.	IV.	V.
1. Chloride of Sodium, . .	13·57	97	·407	·419	157
2. Sulphate of Copper, . .	4·83	60	1·910	3·183	7
3. Ferridcyanide of Potassium, .	2·51	86	·288	·335	42
4. Sulphate of Potash, . .	31·21	63	1·840	2·914	42
5. Ferrocyanide of Potassium, .	8·90	66	1·640	2·485	20
6. Water, . . .	8·93	. .	. .	·991	106

Nos. 2 and 5 are calculated as hydrated crystals.

Column I. gives the expansion of each salt in crystallizing from a

nearly saturated solution of water, the volume in a crystalline state being taken at 100.

Column II. gives the actual pressure in atmospheres in the experiment.

Column III. gives the increased solubility due to the pressure given in column II., the total amount of salt dissolved without pressure being taken at 100.

Column IV. gives the increase in solubility that would be produced by a pressure of 100 atmospheres, as calculated in accordance with the principles already described, the same unite being taken as in column III.

Column V. gives the value of the mechanical work that could be done, or, so to speak, the amount of mechanical force set free when the various substances crystallized from a solution containing 1 per cent. more than would be dissolved without pressure, as measured by the number of times its own weight which any unite of the various salts could rise to the height of 1 metre in the act of crystallization. Conversely, it is the amount of mechanical force which becomes latent in the act of solution; and in the case of a still more supersaturated solution it would be greater, and *vice versâ*, in accordance with the fact of the increased solubility varying with the pressure.

On comparing together the various salts, it will be seen that their properties vary very considerably. Thus, under the same pressure, the extra quantity of sulphate of copper dissolved in nearly ten times that of ferridcyanide of potassium. The mechanical equivalents also vary even more, being (for chloride of sodium) about $22\frac{1}{2}$ times as great as for sulphate of copper. On the contrary, the mechanical equivalents of ferridcyanide of potassium and sulphate of potash are the same; but, under equal pressures, the extra quantity of the latter dissolved is nearly nine times as great, owing to the difference in the amount of expansion in crystallizing. This latter is, however, nearly the same for water and ferrocyanide of potassium, whilst, under the same pressure, the extra quantity of that salt dissolved is $2\frac{1}{2}$ times that of ice, in consequence of the much greater mechanical equivalent of the ice. It appears to me that we may provisionally conclude that the increased solubility due to pressure varies directly with the change of volume, and inversely with the mechanical equivalent of the force of crystalline polarity, so that, if s be the total amount of salt which dissolves without pressure, c be some function of the change in volume in dissolving, and m some function of the mechanical equivalent of the force of crystalline polarity, the solubility, at the same temperature, under a pressure of p atmospheres would be $+ s \frac{p.c}{m}$. If the salt be one that expands on dissolving, c of course is negative, and therefore under pressure the solubility becomes $s - \frac{p.c}{m}$; that is to say, it is diminished, as proved by experiment with sal-ammoniac. If no change in volume took place, we may, I think, also conclude that pressure would not in any way increase or

decrease the solubility of salt. Moreover, since, when a solution is just saturated, the force with which the salt tends to crystallize is equal to that with which it tends to dissolve, their mechanical equivalents must be equal and opposite. Hence we may perhaps conclude that, other circumstances being the same, the mechanical equivalent of a salt like chloride of sodium, which so readily attracts moisture, would be greater than that of one like sulphate of copper, which so readily loses even its water of crystallization; and thus also the relative influence of equal amounts of pressure would be very different, as is confirmed by experiment in the case of these and some other salts.

The facts I have described, therefore, show that there is a direct correlation between mechanical force and the forces of crystallization and solution. According to some chemists, the latter is an instance of real combination; but, whatever views be entertained respecting its nature, we cannot, I think, deny that the force represents some modification of chemical affinity, or is at all events most closely allied to it. In comparison with some kinds of affinity, it may indeed be, and probably is, weak; but, yet as I have shown, it sometimes has a very considerable mechanical equivalent, even when nearly counterbalanced by an opposite force; and since such pressures as glass tubes will resist have no very great influence on what we may perhaps consider a weak affinity, we cannot expect that any pressure at our command would have much influence on strong affinities. I have, however, succeeded in obtaining some results which apparently show that pressure influences undoubtedly chemical changes taking place slowly, and therefore probably due to weak, or nearly counterbalanced, affinities.

The method adopted in this part of the inquiry was to seal up some solid substance in a solution which gives rise to a slow double decomposition, taking great care to have in the tube with pressure, and in that without, pieces cut so as to be of the same size and form, and a solution of the same character, so that, with the exception of pressure, all the conditions were the same. Possibly I may be so fortunate as to discover some case where the affinity is so weak that pressure may determine whether it go forward or not, of which fact the structure of metamorphic rocks furnishes examples; but hitherto I have only been able to prove that pressure modifies the *rate* at which chemical action takes place. This branch of the inquiry is, however, beset with many difficulties, for the change in volume produced by double decomposition is small, and its determination involves several complicated questions. The volumes of the solids is easily determined; but that of the salts in solution is not the same when other salts are present as when they are dissolved in pure water, and varies much according to the strength of the solution and the nature of the salts; and many points are still so obscure, that I shall only give two cases by way of example.

When a portion of Witherite is enclosed in a tube with a strong solution of protochloride of iron, there is a slow decomposition into chloride of barium, which is dissolved, and carbonate of iron, which

remains firmly attached to the Witherite, and would ultmately give rise to an excellent pseudomorph. The best conclusion at which I have been able to arrive is, that there is in the change an increase in volume equal to about 10·7 per cent. of the Witherite altered, so that, under pressure, mechanical force must be overcome. In an experiment where everything went on in a very satisfactory manner, the pressure was maintained for three months at from 80 to 100 atmospheres, and for one month was under 80 atmospheres, so that, on an average, it was about 80 atmospheres; and I found that the amount of chemical change was 21·7 per cent. less than when, all other circumstances having been the same, there had been no pressure; thus clearly showing that pressure had, as it were, diminished the force of chemical affinity. If then one cubic inch had been altered under this pressure, it would have overcome a mechanical force equal to that required to raise 1200 lbs. through the space of ·107 inch, which is equivalent to raising twenty-one times its weight to the height of 1 metre; and under the same circumstances 1·278 cubic inch would have been altered when no such mechanical force had to be overcome. Supposing then that in both cases the total energy at work was the same, but in one was altogether expended in producing a chemical result, and in the other in producing partly a chemical and partly a mechanical effect, we may say that the force which gives rise to the purely chemical change, taking place at a particular rate, is equal to that which gives rise to this chemical effect, taking place at ·783 of that rate, and to a mechanical effect equal to the force required to raise in the same space of time 34·87 times the weight the Witherite altered to the height of 1 metre. Supposing also that the power of chemical force varies at the rate at which it gives rise to a chemical change, in the same manner as the power of a mechanical force varies as the velocity of motion imparted by it, we may perhaps conclude that this mechanical force is equal to ·217 of the chemical force, and that the whole energy of the chemical action under the conditions of the experiment was equal to the mechanical power required to raise in the same period of time 160 times the weight of the Witherite altered to the weight of 1 metre. If these principles are correct, a pressure of more than 370 atmospheres would have entirely counterbalanced the force of chemical affinity, since to produce any chemical change it would then have had to overcome a greater force than it possessed. This is so great a pressure that I fear it will be difficult to prove the deduction by experiment; and until some such case can be found, capable of being verified, these calculations must be considered as little more than suggestions, which future investigations may confirm or disprove.

When calcite is sealed up in a mixed and rather strong solution of chloride of sodium and sulphate of copper, slow double decomposition gives rise to malachite, sulphate of lime, and carbonic acid; and though this is extremely complicated, and it is very difficult to determine what would be the change in volume, yet, so far as I am able to make out, until the solution becomes saturated with sulphate of lime,

there is a decrease in volume equal to about 8 per cent. of that of the calcite altered, so that, under pressure, mechanical force is the very reverse of being opposed to the chemical change. Three experiments all indicate the same fact, and in which, on an average, the pressure was about 90 atmospheres for two weeks, show that, as a mean of the whole, the amount of chemical change of 17 per cent. more with the pressure than without; thus proving that pressure had, as it were, increased the force of chemical affinity. Calculating according to the principles described above, we may conclude that a pressure of 530 atmospheres would have caused the action to take place at double the rate, and that therefore the chemical action is equivalent to the expenditure of that amount of mechanical force, being thus generated by it. Arguing then in a manner similar to that already described, but modified to suit the different conditions, if there be a contraction equal to 8 per cent. of the bulk of the calcite, there must be a loss of mechanical force capable of raising 28 times the weight of the calcite altered to the height of 1 metre, in the time required for the chemical change; which amount of mechanical energy, as it were, becomes latent, and is transformed into chemical action, and would again exhibit itself as a mechanical force if, by any means, the chemical affinities could be inverted and everything restored to its original state.

In a like manner, other experiments indicate that in some cases pressure causes a slower, and in others a quicker chemical action, whilst in others it has scarcely any influence whatever; and though, for reasons already explained, I say it with some hesitation, yet bearing in mind what is already known respecting the action of pressure on hydrate of chlorine, hydrated hydrosulphuric acid, and other substances described by the various authors referred to in the notes, I think the facts I have described make it very probable that further research will show that pressure weakens or strengthens chemical affinity according as it acts against or in favor of the change in volume; as if chemical action were directly convertible into mechanical force, or mechanical force into chemical action, in definite equivalents, according to well-defined general laws, without its being necessary that they should be connected by means of heat or electricity. On the present occasion I shall not attempt to consider the various geological and mineralogical facts which appear to me to admit of the application of the principles I have described, for many of them are peculiarities in structure of which neither myself nor any one else has ever given a description, and would therefore demand a preliminary notice. However, I may say that it appears to me that a number of facts connected with metamorphic rocks and the phenomena of slaty cleavage, which, to me at all events, have hitherto been inexplicable, are readily explained if mechanical force be directly correlated to chemical action, and if in some cases the direction in which crystals are formed be more or less related to pressure, in some such way as there is a connexion between their structure and the magnetic force shown by the experiments of Plücker, Faraday, Tyndall, and many other observers.

We may also, I think, explain the origin of the impressions on the limestone pebbles in the "Nagelflue" in Switzerland, about which so much has been written in Germany and France, without a satisfactory reason having been discovered; and the same explanation accounts for the mutual penetration of the fragments of which some limestones are formed, and for the banded structure of some which possess slaty cleavage. The curious teeth-like projections with which one bed of limestone sometimes enters into another, also to a certain extent indicate a chemical action depending on mechanical force; and probably the same may be said of some of the peculiarities of slickensides and mineral veins. It is also possible that a pressure of several hundred atmospheres may facilitate some of the chemical changes involved in the transformation of water and carbonic acid into the organic compounds met with in animals and plants of low organization found at great depths in the ocean, and thus to a certain extent compensate for diminished light. I, however, most willingly admit that very much remains to be learnt before we can say to what extent the principles I have described are applicable; and yet, at the same time, cannot but think that henceforth they must be taken into account in many departments of the chemical and physical geology, and will readily explain a number of facts which otherwise would be very obscure.

On the Measurement of the Chemical Brightness of various portions of the Sun's Disk. By HENRY ENFIELD ROSCOE, B.A., F.R.S.

From the Proceedings of the Royal Society, No. 56.

The author has applied the method of measurement of the chemical action of sunlight, which Professor Bunsen and he described in a memoir presented to the Royal Society in November last,* to the measurement of the chemical brightness of various portions of the solar disk; and although the observations which have as yet been made, are only preliminary, yet he thinks that the results obtained are of sufficient interest to warrant his bringing them before the Society.

Secchi has shown† that the calorific radiation of the centre of the sun's disk is nearly double that from its borders, and that the equatorial regions are somewhat hotter than the polar, whilst observers have long noticed a great difference in luminosity between the centre and edge of the disk.

For the purpose of obtaining a measurement of the relative chemical brightness of various portions of the solar disk, the image of the sun, of about 4 inches in diameter, obtained by a $3\frac{1}{2}$ inch refractor,‡ was allowed to fall into a camera placed on the instrument, upon a sheet of standard photographic paper prepared according to the method described in the above-mentioned research. The peculiar property of this standard paper is that it can always be prepared of one and the same degree of sensitiveness, and is perfectly homogeneous. The exposure

* Abstract, Proc. Roy. Soc. vol. xii. p. 306; Memoir, Phil. Trans. 1863.
† Astron Nachr. Nos. 806, 833.
‡ Kindly placed at my disposal by S. W. Williamson, Esq., Manchester.

lasted for from 30 to 120 seconds, the sun's motion being carefully followed by a tangent-screw. After exposure, the shade of tint at several points on the picture was determined by comparison with a graduated photographic strip insolated in the pendulum-photometer, and the chemical intensities corresponding by these shades obtained by reference to the Table given in the memoir above cited. The following numbers give the chemical brightness, thus obtained, at various points on the sun's disk on May 9th, 1863. From these numbers it is seen that the intensity of the chemically active rays at the centre is from three to five times as great as that at the edge of the disk, the chemical rays thus showing a wider variation than the calorific rays exhibited as determined by Secchi. This is doubtless owing to the relatively greater absorption effected by the solar atmosphere on the more refrangible chemical rays.

Chemical Brightness of Sun's Disk on May 9th, 1863.

	1. At centre of Sun's Disk.	2. At 15° from the edge of Sun's Disk.			3. At edge of Sun's Disk.		
		N. Pole.	Equator.	S. Pole.	N. Pole.	Equator.	S. Pole
No. 1.	100·0	38·8	48·4	58.1	18·7	30·2	28·2
No. 2.	100·0	52·8	. .	56·6	30·5	. .	41·0

Hence it is likewise seen that on May 9th the chemical brightness of the south polar regions was considerably greater than that of the north polar regions, while about the equator the brightness was between that of the poles.

In order to show that the sensitive paper, when exposed to ordinary sunlight, becomes homogeneously tinted, the author appends the readings, taken in the way described, from various portions of a piece of the standard paper used for the sun-pictures exposed for some seconds to direct sunlight.

	Reading.	Deviation from mean.
Portion No.	1, 101·4	+0·93
"	2, 100·7	+0·23
"	3, 98·5	—1·97
"	4, 101·6	+1·13
"	5, 99·9	—0·57
"	6, 100·7	+0·23
	Mean, 100·47	

The sun-pictures obtained on the sensitive paper must possess only a slight tint, otherwise the differences in shade cannot be accurately observed; they then exhibit a peculiar coarse mottled appearance, which is not due to imperfections in the paper, or the lenses, nor to the action of the earth's atmosphere.

Perhaps these irregular dark and light patches are owing to clouds in the solar atmosphere, and they may have an intimate connexion with the well-known phenomenon of the red prominences.

Mr. Baxendell and the author propose to carry out, according to

this method, a regular series of observations of the variation of the relative amounts of brightness on the sun's disk, and they hope before long to be able to present the Society with some further details.

Note on Vegetable Ivory. By Dr. Phipson, F.C.S., &c.

From the London Chemical News, No. 206.

Vegetable ivory is the fruit of *Phytelephas macrocarpa*, a plant allied to the palm trees, common in South America. At the period of maturity the grain forms a hard mass resembling ivory or bone, and which is manufactured into various kinds of ornaments. According to an analysis by Mulder, its composition may be represented by—

$$C_{24}H_{21}O_{21}=^{2}C_{12}H_{10}O_{10}+HO;$$

Baumhauer obtained a precisely similar result some years later.

I have found that vegetable ivory takes, in contact with concentrated sulphuric acid, a splendid red color, almost equal to magenta. This color, at first pink, then bright red, becomes much deeper and more purple when the acid has been allowed to act for about twelve hours.

This reaction may sometimes be found useful in order to distinguish small pieces of vegetable ivory from the ivory of the elephant's tusk, or from bone, neither of which take this beautiful red color in contact with sulphuric acid.

The analyses quoted above show that the greater portion of vegetable ivory is pure cellulose, but the reaction produced by sulphuric acid proves that other substances are present, for cellulose does not become red with sulphuric acid. Mr. Connel found in 1845, that vegetable ivory contained 81·34 per cent. of cellulose, and that the other substances were gum 6·73, legumine 3·80, albumine 0·42 (that is 4·22 of albuminous substances), oil 0·73, water 9·37, and ash 0·61,=100. Filings of vegetable ivory dried at 140° to 150° C. give 1 per ct. of ash.

Payen found that these filings when boiled with caustic soda took a yellow color, a fact confirmed by Baumhauer, who asserts that potash does not produce any color.

The reaction of sulphuric acid on vegetable ivory has enabled me more than once to distinguish immediately between filings of this substance, and bone of ivory filings. It is owing to the well-known action of this acid upon albuminous substances in presence of sugar, and which has been utilized by Raspail in his microscopic researches. But whether the sugar is formed by the action of the acid in the cellulose, or pre-exists already formed in the substance is of little import. I incline, however, to the first opinion, as the color takes a little time to show itself (five or ten minutes), and as Mr. Connel did not find any sugar ready formed.

I have since observed that the white portion of the cocoa-nut presents a similar reaction with sulphuric acid; the color produced is first pink, then red, reddish purple, and finally, in about sixteen hours, a fine violet.

The colors thus produced with vegetable ivory and cocoa-nut disappear gradually in contact with water, like the fine reddish-brown color produced with essence of turpentine and sulphuric acid.

Gun Cotton as an Explosive Agent.

From the London Practical Mechanic's Journal, October, 1863.

(Continued from page 46.)

Gunpowder and Gun Cotton compared as to Temperature of Ignition.—Nevertheless, it is of interest to introduce something regarding the qualities of gunpowder. Mixed with sulphur, charcoal ignites at a lower temperature than by itself. A mixture of sulphur with charcoal, that had been prepared at temperatures between 150° and 400° C., begins to ignite at 250°; whereas a mixture of sulphur with charcoal, resulting from a temperature between 1000° and 1500°, when heated to 250° only burns off the sulphur, without the combustion of the charcoal.

Charcoal decomposes saltpetre at a temperature altogether different from the degree of heat at which charcoal is produced. Charcoal prepared at temperatures between 150° and 430° decomposes saltpetre at 400°. Charcoal prepared between 1000° and 1800° decomposes saltpetre at a dark red glow heat.

Sulphur decomposes saltpetre at higher temperatures than the charcoal, namely, at about or over 432°. The temperature at which sulphur burns in the air is, according to Violette, 250°, and at this temperature gunpowder also explodes. The explosive quality of various sorts of gunpowder is different, according to the sort of charcoal used, the proportions of the mixtures, and the size and compactness of the grains. Violette observes, that an exact knowledge of the alterations in the composition of the charcoal of powder, in proportion to the temperature of the charcoaling process, must be followed by changes in the respective proportions in the mixture of the gunpowder. It is useless to advert here to the conditions under which charcoal is here produced by the common charcoal burners.

Is Gun Cotton Spontaneously Combustible?—Finally, there still remains the consideration, whether the danger of the spontaneous combustion of gun cotton is so great that it cannot be recommended for shooting purposes.

Spontaneous combustion is well defined by Lieut. Col. Baron von Ebner, his definitions having reference to the military conditions.

Whence, then, have arisen the allegations of spontaneous combustion, and on what basis do they rest? Assuredly, they receive no countenance from the experience acquired by chemists, who in preparing gun cotton have each operated in a different manner. Pelouze maintains that the process of azotizing is finished in a few minutes. The explosive nature of the compound of this chemist, as well as that of Boucher, is therefore attributable to a composition wholly different from that of the Hirtenberg cotton.

Nor are apprehensions of spontaneous combustion deducible from any fair application of theory. Whatever theoretical objections there may be at the present time applicable, were also applicable twelve years ago. In the summer of 1862, the Simmeringer Haide explosion occurred, and we may fairly assume that the list of theoretical assumptions favoring the idea of spontaneous explosion was then pretty well

exhausted. In the Simmeringer Haide magazine, however, gun cotton and gunpowder were stored together. To have imputed the explosion to spontaneous ignition of gun cotton, was altogether unwarranted by evidence. With equal show of probability might it have been imputed to the spontaneous ignition of gunpowder.

The latter view might be supported by the experience of numerous powder explosions where not an atom of gun cotton was present, and yet the cause was never ascertained.

Many probable reasons, however, may be adduced favoring the belief that the Hirtenberg cotton did not explode spontaneously; these probabilities rest on grounds both theoretical and practical.

Experimental Proofs demonstrate that Lenk's Gun Cotton is not spontaneously Combustible.—The history of gun cotton as chronicled by chemists and artillerists, short though the history be, is so full of records of explosion under unexpected circumstances, that an unbiassed mind can hardly fail to be impressed with the belief that amongst the ordinary conditions of military practice, there may be some competent to induce the spontaneous combustion of this material. Nevertheless, the experience of Baron Lenk, acquired during a period extending over more than ten years, is more pregnant with reliable testimony than can be found in the entire remaining history of this material.

The *manufacture* of *gun cotton* in *Hirtenberg* consists in a *number* of *perfectly harmless operations;* and it is remarkable that contrary to what happens with gunpowder, if fire be not actually applied, explosion is impossible. All operations are so arranged, that the material acted upon is in a moist or wet condition—hence not explosive. Drying takes place in a capacious building, on every side open to the air. The last process of drying is carried out in the drying chamber, where it is effected by a stove situated on the outside, distributing its heat to the building by earthenware pipes; drying being thus insured through a gentle warmth. The gun cotton next goes either into a magazine to be packed away in chests, or is at once prepared for ammunition.

In *this magazine, Hirtenberg* cotton has been stored for a period of twelve years, and not a single instance of explosion has taken place. How many powder mills have exploded in that time? In Prussia, however, a drying chamber has lately blown up.

Your excellency has officially been informed, that in Prussia they have worked for eight years with gun cotton, and not a single explosion has occurred except the last named. In the Prussian drying chamber referred to, a stove with an iron smoke pipe was used—a sufficient explanation of the misfortune.

During twelve years we have prepared gun cotton at Hirtenberg, for ammunition, that is for yarns, spun ropes, and threads twisted and woven.

One single case of explosion has occurred in the course of Baron Lenk's manufacture, the result of improper speed of working the spinning machinery. Now, the circumstance hardly need be insisted on, that gunpowder as well as gun cotton can be exploded by friction.

Gun cotton has been used for military purposes now more than twelve years; it has also been employed for mining and blasting. It has been subjected to every variety of transport. Packed in black wooden chests, it has been exposed to sunshine for months together; all this without one single accident. In the face of such testimony it cannot be said that gun cotton manifests any tendency to explode spontaneously.

Theoretical Proofs against the Spontaneous Combustion of Lenk's Gun Cotton.—Those who maintain the spontaneous combustibility of gun cotton, must surely be able to base their fears on scientific grounds. Let us now examine the most important of these. And, firstly, as regards cotton itself. Cotton is chemically effected by storage, if packed damp and in bulk; it then gets heated; and examples are not wanting of warehouses burned down through fire generated by the heating of stored cotton. This chemical change in ordinary cotton begins with hydrogen, this element affording to the atmospheric oxygen the first point of attack: hereby occurs heating, with partial oxidation. The oxidation goes on until even the charcoal is affected, and the results may be actual combustion. This occurs not only to cotton, but linen rags, as well as to sawdust, and many other organic substances. The conditions affecting Lenk's gun cotton are almost diametrically opposed to the preceding. Firstly, the three easily removable atoms of hydrogen present in ordinary cotton are disposed of by what may be called azotization; then the long process of acid-steeping involved, oxidizes everything in the cotton that can be azotized; whence it follows that there remains nothing for atmospheric oxygen to act upon. Finally, slow decay, or "*eremacausis*," to which ordinary cotton is subject, cannot affect Lenk's gun cotton.

Granting that atmospheric oxygen has no influence, still the theoretical allegation may be adduced, that oxygen of water is not without influence. But the Hirtenberg gun cotton lies for several weeks in water; moreover, is finally boiled with weak potash solution. Now, if it be easily changeable by water, it must surely have changed under this treatment: nevertheless, every analysis demonstrates the result to be *tri-nitro cellulose*. It may still be argued, that if oxygen and atmospheric moisture do not produce material changes, still summer heat would do so; but examples such as the black cases which have lain for months in the open air on railway stations, exposed to the direct action of the sun, and the specimen already referred to, where more than one pound of gun cotton was exposed for more than four weeks to the direct action of the sun towards the south in all weathers, retaining still its full explosive properties, demonstrate a sufficient explosive constancy. If, however, gun cotton be prepared otherwise than as in Hirtenberg—or if not prepared in Hirtenberg, according to the special instructions of General von Lenk (of which, until now, no example was before us)—then, no doubt, there might be risk of combustion, as in the case of the French cotton.

How the explosions of the French cottons are to be accounted for.—The explosion can be accounted for in the following way: indeed, it has been mostly explained already.

If the acids are not strong enough, or not permitted to act sufficiently long; if, moreover, the subsequent removal and neutralization of acid be not complete, so that free acids remain; then the atoms of unremoved hydrogen may become the focus of chemical energy, which rising to a sufficient degree of intensity, may result in explosion.

To effect complete permeation of the cotton, is by no means a quick or easy process; and a similar remark is applicable to the removal of lingering acid traces. With reference to these particulars, the statement can, without arrogance, be made that the French gun cotton, compared with that of Hirtenberg manufacture, was not so complete a material, inasmuch as the acid was allowed too short a time for its action upon the cotton wool; probably, too, the subsequent *deacidation* of the French cotton would not have been so fully effected. Thus considered, the assumed great danger of so-called "spontaneous combustion" in reference to Lenk's gun cotton disappears.

It now remains, lastly, for us to inquire into the so-called disruptive explosibility (*vis viva*, or *force brisante*) of gun cotton.

The "force brisante" of Gun Cotton.—This subject, purely artilleristic in its nature, can only be spoken of by chemists, so far as the *force brisante* of gun cotton depends upon a certain chemical condition.

By the expression *force brisante*, we understand the time within which a certain weight of an explosive material in a gun has been converted into a certain volume of gas, or the volume of gas produced under otherwise equal conditions.

An explosive material is therefore the more *brisante* in proportion as the time is short for equal weight and volume of gas developed, or time and weight equal in proportion to the volume of gas is greater.

We, being accustomed to compare every sort of explosive material with gunpowder, say that gun cotton is more *brisante* than gunpowder, because the former develops an equal volume of gas in a smaller space of time; or what is the same, time being equal, a greater bulk of gas, and, therefore, a higher mechanical effect. In the moment of explosion the result exerts a greater pressure on the barrel; hence there is more danger of bursting.

The danger of bursting by equal charges differs relatively according to the thickness of the barrel and the strength of its material to equal resistance of the shot.

Given an explosive substance more *brisante* than gunpowder, we can still fire the two with equal safety by proportionately increasing the strength of the gun. From a consideration of explosive bodies generally, it follows that each particular one, if applied to ballistic purposes demands a peculiar gun. It cannot be reasonably expected that a gun best adapted to the use of gunpowder is also best adapted to the use of gun cotton.

If that construction be discovered, then the higher *brisante* effect of gun cotton may appear as an advantage, instead of being thought a disadvantage.

However, gun cotton can act disadvantageously in another manner on the barrel of a gun, through its higher *brisante* nature.

If we imagine the moment of the discharge of the gun taking place, then the products of the explosion—the gases, are as it were, in a mine, in which one of the resisting sides is movable, viz: the shot. The gases act with equal mechanical power on every side of the barrel in the moment before the shot is about to move. The sides of the gun being at rest (supposing it resists the explosion), it follows that part of the mechanical power is transformed in an equivalent quantity of heat, or, what is the same, the barrel of the gun will become heated; the temperature may be raised thereby so high, that the tin of a bronze barrel may partly melt, or, as we say, the barrel of the gun is "burnt out" after a number of rounds.

That the last-named effect of heat on the barrel is not to be attributed to the chemical composition of gun cotton, but to the transformation of mechanical power into heat, is proved by Lieutenant von Karoly's analysis of the gases of combustion of Lenk's gun cotton, which he made in the Chemical Laboratory of the Engineers' Corps Committee, as may be seen in the report of the Imperial Academy of Science, volume xlvii, page 59, Mathematical and Physical part; and which may be seen in the following table, in which the gases of combustion of powder according to Bunsen (*vide* Poggendorf, 4th series, 12th volume, page 131,) are cited in comparison with those of gun cotton.

Gases of Combustion. Volume per cent.		Bunsen.			Karoly's.	
		Sporting.	Rifle.	Common Powder.	Gun Cotton.	
Nitrogen,	N.	41·1	35·3	37·6	.	12·7
Carbonic Acid,	$C.O_2$	52·7	48·9	42·7	.	20·8
Carbonic Oxide,	C.O.	3·9	5·2	10·2	.	29 0
Hydrogen,	H.	1·2	6·9	5·9	.	3·2
Sulphur and Hydrogen,	H.S.	0·6	0·67	0·86	Carbon,	1·8
Oxygen,	O.	0·55	. .	. .	Water,	25·37
Light Carburetted Hydrogen Gas,		0·00	3·02	2·7	.	7·2

If we compare the gases of gunpowder with those of gun cotton, we easily see that the chemical action of the product of combustion of gun cotton on the sides of the barrel—if there exists any action at all—must be smaller than with the use of gunpowder, because they are less oxidizing gases than those of gunpowder.

Should, therefore, bronze barrels be "burnt out" by the use of gun cotton, cast steel may be then used instead of bronze, which, in fact, has been successfully done.

Moreover, bronze gun barrels have withstood a sufficient number of rounds by using an adequate charge of gun cotton with elongated cartridges. In this way no alteration of the bore prejudicial to the correctness of aim has taken place.

From the steel barrel of a rifle, forty rounds have been fired with

gun cotton cartridges, which have hit the target 300 yards distant in an unexceptionable manner. After the said number of rounds, the barrel was internally as clean and polished as a mirror. It appears, then, that this problem is solved in a general and satisfactory manner.

Application of Gun Cotton to civil mining and in warfare.—Gun cotton is also used for mining purposes and mining warfare. On this subject nothing but what is favorable has been reported by the Imperial Engineers (*vide* communications of the R. K. Engineers' committee, 1861, 1st volume, by Moritz Baron von Ebner, Colonel of the Engineers).

However, it is said that the gases of gun cotton were more poisonous in mines than those of gunpowder, and, therefore, the use of gun cotton for mining warfare is not to be recommended.

If we compare the results of Lieutenant Karoly's analysis of the combustion gases of gun cotton with those of gunpowder as above given, we observe that both of them contain irrespirable gases; further that they contain qualitatively the same sort of irrespirable gases; and although the relative quantities of some of the gases from powder and gun cotton are different, the effect of those gases leads to the same practical result, viz: that after blowing up a mine, one cannot without danger approach the spot of the explosion before renewing the air by ventilation.

In this respect we may say that the gases of gun cotton will be more quickly removed by ventilation than those of gunpowder, because the first named contain a greater quantity of gases easily dissipated; since 100 pounds of gunpowder contains 68 pounds of fixed solid matter, which alone suffices to make respiration almost impossible.

It is not probable that an explosive compound will be found which will produce any other but irrespirable gases.

It is one and the same in practice, whether a cellar contains 40 per cent. of carbonic acid and 10 per cent. carbonic oxide, or 30 per cent. carbonic oxide and 20 per cent. carbonic acid, inasmuch as no one could without danger of suffocation, enter such a cellar.

Both the gases of gun cotton and of gunpowder, according to Karoly's, may be ignited by a match.

Recapitulation and conclusions.—We believe we have in the preceding furnished replies to your excellency to all the important questions which chemists may be called upon to decide prior to determining whether or not General Lenk's gun cotton can be used as an explosive material for warfare; and we will conclude our report with the following general remarks:—

General Baron von Lenk's gun cotton is almost wholly made up of tri-nitro cellulose; it is manufactured in the imperial factory at Hirtenberg in such manner that one invariable product results.

Lenk's gun cotton remains unaltered under circumstances that would render gunpowder totally useless. It is not subject to any notable alteration, nor is it prone to spontaneous combustion. Its temperature of ignition is somewhere between 130° and 160° C., sufficiently high to remove apprehension. Its *vis viva*, or *force brisante*, can be moderated

without difficulty if required, or else special cannon and rifle barrels may be adapted to it.

The dangers in regard to mining warfare are comparatively and qualitatively the same as with the use of gunpowder,—the gases of both being irrespirable.

The manufacture of gun cotton at Hirtenberg is provisional, and General Baron W. von Lenk will know best how to devise improved mechanical apparatus when the erection of a proper factory shall be resolved upon.

According to our experience up to the present time, we recognise in the *gun cotton from Hirtenberg an improved explosive compound having many and great advantages, several of which gunpowder from its very nature can never possess.*

A Novel Application of Water Power.

From the London Mechanics' Magazine, October, 1863.

Just forty years since, M. Fourneyron commenced a series of experiments in water power, which resulted in his invention of the turbine or horizontal water-wheel. Since that period considerable improvements have been made in the turbine by different persons, the chief and most useful having been effected by Mr. Schiele, of Manchester, whose ingenious applications of mechanical curves seem to have been fully adapted by him for the production of this form of motive power. One form of his arrangement for supplying power we have recently seen (working the bellows of a powerful organ) at the residence of a citizen of Manchester, where the impression was given that, if all the results achieved by Mr. Schiele be equally successful, a new feature will be rapidly developed in applying water power, especially in cases where a small amount of power may be required at irregular periods; as in the case of working the bellows of organs, driving small lathes, fans for ventilation, printing and other presses, sewing machines, washing machines, &c. In the house referred to, a water-wheel, 4 feet in diameter, consuming 15 gallons of high-pressure water per minute, formerly employed to work the bellows of an organ in the drawing-room over the cellar wherein the water power was produced, has been replaced by a turbine only $1\frac{1}{4}$ inches in diameter, with a 3-inch case $1\frac{1}{4}$ inches wide, supplied by a $\frac{3}{8}$-inch pipe, and consuming less than a gallon of water per minute. An ingenious and yet very simple economical regulator, invented by Mr. Eccleston, organ builder, of this city, works in connexion with the apparatus just mentioned, by means of which the organist may easily supply his instrument with the required wind by simply turning a handle near the organ. By availing themselves of the ample supply of high-pressure water secured to the city by our Corporation, all persons using machines requiring a small amount of power appear now to have supplied to them by this invention the means of working their machine with no trouble and at a trifling cost; while at the same time this kind of turbine appears to be equally well adapted for turning large mills and works, even when they require several hundreds of

horses' power. Orders are now being executed by Messrs. Schiele and Co. for the construction of 50 small turbines, to be used as direct-action fans (the turbine and fan being on one spindle) for the production of the new gas obtained from petroleum. Several powerful turbines will shortly be at work in this locality, when our readers will be able to see and judge for themselves of the extraordinary yet simple effect of this new water engine, which seems to be equally suited for the requirements of the sewing machine in a lady's boudoir, the washing machine and mangle in the laundry, or the hydraulic press and hoist in our huge ware-houses. In fact, wherever our Corporation water-works will enable persons to turn a water tap, and thus to supply at a moment's notice the power required, these machines will be available; while all the risk from fire and the cost and trouble of steam boilers and engines will be avoided.—*Manchester Guardian.*

Extraction of Iron.

From the London Chemical News, No. 206.

To the Editor of the Chemical News.

SIR:—I find in your paper of October 31, an article by M. A. L. Fleury on the "Extraction of Iron from Cinders of Puddling and Reheating Furnaces," and as the statements therein contained might mislead iron masters into the belief that his invention is a new one, I deem it my duty to address to you the following remarks:—

In the year 1854 I devoted much time to ascertaining the best methods of employing cinders from puddling and refining furnaces, so as to enable iron masters to use them in larger proportion on their blast furnaces without injury to the quality of the iron produced; and in August of the same year I took out a patent to carry out my views, which is identical with that of M. Fleury. In my specification I stated "The slag or cinder, either before or after being calcined or roasted, is ground into coarse powder and mixed with half its weight of slacked lime, and made into a thick paste, which is formed into lumps or bricks with or without coal-dust or charcoal, and is introduced, dried, or calcined or not, into the blast furnace, and worked in the usual way."

I soon found that this process was not practicable, owing to the enormous expense of grinding the cinders, and afterwards mixing them with slacked lime and making the whole into bricks, and lastly, because the constant vibration which exists in blast furnaces, owing to the working of the blast engine, caused the bricks to fall into powder and to interfere with the draft, and consequently to impede the working of the furnaces.

Therefore, in September, 1855, I took out a second patent for fusing the cinders with about 20 per cent. of quicklime, 30 per cent. of slacked lime, or 50 per cent. of carbonate of lime, previously to using the cinders on the blast furnace. This process I carried out with perfect success on the continent and in South Wales; by this process the silica, sulphur, or phosphorus contained in the slag are retained by the lime, and pass away in the cinder of the blast furnace, whilst

the oxide of iron thus liberated is reduced with great facility to a metallic state. I am aware that cinder or slag and lime had been used on the blast furnace for a long period, but my improvement consists in combining cinders and lime in proper proportions previously to placing them on the blast furnace, thus securing a perfect combination of the cinder with its flux, so that when it comes into contact with the reducing agents in the blast furnace, the oxide of iron is at once liberated from the impurities with which it is united.

As to the value of chloride of sodium for purifying iron, I would also refer M. Fleury to a patent which I secured on October 30, 1851, for the use of chloride of sodium as a means of removing sulphur either from iron ores or from cokes. The value of this process was fully tested on a practical scale at several iron works, and also in a series of experiments conducted at the establishments of the following eminent engineering firms, with results which I append :

Experiments conducted at various works showing the comparative bearing strength of iron bars one inch square, four feet six inches between the bearings, melted by the ordinary and by my purified coke. These experiments were upon various qualities and descriptions of iron, and varied in every possible way, in order that the results might be relied on.

At the Works of	Mean breaking weight of bars melted by		Difference in favor of purified Coke.
	Ordinary Coke.	Purified Coke.	
	lbs.	lbs.	per cent.
Fairbairn and Sons, Manchester, .	427	512	20
J. & W. Galloway, " .	547	620	13
Fox, Henderson & Co., Birmingham, No. 3 iron,	435	514	18
" " " " No. 2 "	317	459	10
Hibbert, Platt & Co., Oldham, .	399	552	10
Monkland & Co., . . .	578	641	10
Ivicey & Co., Skinnerburn, .	616	*741	20
Newcastle, ...	658	*716	9
Elswick Foundry, . . .	695	*863	24

Messrs. Fox, Henderson, and Co. paid particular attention to the quantity of the gas given off during the process of casting the bars, and report as follows :—

" We could easily smell the sulphur given out from the top of the cupola, and also from the tapping hole, in which ordinary coke was used, but there was no smell whatever, save a little of salt, from the one which had patent coke in it."

* Were bars of three feet ten inches and four feet only in length.

Experiments made at the Victoria Works, Ebbw Vale, for the purpose of testing the yield of metal smelted with the ordinary process, and by Mr. Calvert's improvement.

By ordinary process.

	Charges.	Yield tons.
No. 3 furnace, week ending June 19, . .	481	160
" " " 26, . .	468	174
" " July 3, . .	472	182
By Mr. Calvert's process.		
Week ending July 10, ...	458	183
" " 17, ...	492	197
" " 24, ...	488	201
" " 31, ...	496	202

(Signed) THOS. HARRIS.

I am, &c. F. CRACE CALVERT.

Royal Institution, Manchester.

On Aluminium. By Mr. I. L. BELL.

From the London Athenæum, Sept. 1863.

The author said—"The progress of the manufacture of this, so far as the arts are concerned, new metal has scarcely been such as to require much to be added to the researches bestowed upon the process by the distinguished chemist, M. St. Cairi Deville, of Paris. Upon the introduction of its manufacture at Washington, three years and a half ago, the source of the alumina was the ordinary ammonia alum of commerce, a nearly pure sulphate of alumina and ammonia. Exposure to heat drove off the water, sulphuric acid, and ammonia, leaving the alumina. This was converted into a double chloride of aluminium and sodium by the process described by the French chemist and practised in France, and the double chloride subsequently decomposed by fusion with sodium. Faint, however, as the traces might be of impurity in the alum itself, they, to a great extent, if not entirely, being of a fixed character when exposed to heat, were to be found in the alumina, from which, by the action of the chlorine on the heated mass, a large proportion, if not all, found their way into the sublimed double chloride, and once there, it is unnecessary to say that under the influence of the sodium, any silica, iron, or phosphorus found their way into the aluminium sought to be obtained. Now, it happens that the presence of these impurities in a degree so small as almost to be infinitesimal, interferes so largely with the color as well as with the malleability of the aluminium, that the use of any substance containing them is of a fatal character. Nor is this all, for the nature of that compound which hitherto has constituted the most important application to this metal —I mean aluminium-bronze—is so completely changed by using aluminium containing the impurities referred to, that it ceases to possess

any of those properties which render it valuable. As an example of the amount of interference exercised by very minute quantities of foreign matters, it is, perhaps, worthy of notice that very few varieties of copper have been found susceptible of being employed for the manufacture of aluminium-bronze; and hitherto we have not at Washington, nor have they in France, been able to establish in what the difference consists between copper fit for the production of aluminium-bronze, and that which is utterly unsuitable for the purpose. These considerations have led us, both here and in France, to adopt the use of another raw material for the production of aluminium, which either does not contain the impurities referred to as so prejudicial, or contains them in such a form as to admit of their easy separation. This material is Bauxite, so called from the name of the locality where it is found in France. The Bauxite is ground and mixed with the ordinary alkali of commerce, heated in a furnace. The metal is so extensively used in the arts as to keep the only work in England, namely, that at Washington, pretty actively employed. As a substance for works of art, when whitened by means of hydrofluoric and phosphoric acid, it appears well adapted, as it runs into the most complicated patterns, and has the advantage of preserving its color, from the absence of all tendency to unite with sulphur or become affected by sulphuretted hydrogen. A large amount of the increased activity in the manufacture referred to is due to the exceeding beauty of its compound with copper, which is so like gold as scarcely to be distinguishable from that metal, with the additional valuable property of being nearly as hard as iron."

Proceedings British Association.

On the Expansive Energy of Heated Water. By W. J. Macquorn Rankine.

(Read to the Institution of Engineers, Scotland.)

From the Lond. Civ. Eng. and Arch. Journal, Nov., 1863.

As the question of the quantity of mechanical energy which a given weight of water, heated to a given temperature, is capable of exerting in the act of partially evaporating without receiving heat, until it falls to a given lower temperature, has been raised in connexion with recent researches as to the bursting of steam boilers, I may point out that the complete solution of that question for any given liquid, together with a numerical example in the case of water, is given under the head of Proposition XVII, of a paper on Thermodynamics, which was communicated to the Royal Society in December, 1853, read in January, 1854, and published in the "Philosophical Transactions" for 1854.

That solution is expressed by the following formula (page 161, equation 65):—

Energy exerted by each pound of fluid

$$= \kappa \left\{ t_1 - t_2 \left(1 + \text{hyp.log.} \frac{t_1}{t_2} \right) \right\} \quad . \quad . \quad (1)$$

in which κ denotes the dynamical values of the specific heat of the

liquid, being the product of its specific heat expressed in the ordinary way by "Joule's Equivalent;" t_1 and t_2 the initial and final absolute temperatures; the absolute zero being 274° Centig., or 493°2 Fahr. below the melting point of ice.*

Another equation (equation 63 of the paper) gives the following value for the excess of the final volume to which the mixed liquid and vapor expand, above the original volume of the liquid:

$$\frac{\text{K hyp. log.}\frac{t_1}{t_2}}{\frac{dp_2}{dt_2}} \quad . \quad . \quad . \quad . \quad (2)$$

in which $\frac{dp_2}{dt_2}$ denotes the rate at which the pressure, in pounds on the square foot, varies with temperature at the final temperature.

When applied to the water in a steam boiler, these equations take the following form:—

The value of K for liquid water is 772 foot-pounds per degree of Fahrenheit in a pound of water, or

1389·6 foot-pounds per Centigrade degree in a pound of water, or

423·55 kilogrammetres per Centigrade degree in a kilogramme of water.

The final absolute temperature is 212° F. + 461°·2 = 673°·2 F.

The corresponding value of $\frac{dp}{dt}$ for Fahrenheit's scale and British measures, is 42: and 772 ÷ 42 = 18·38.

Let T denote the initial temperature on Fahrenheit's ordinary scale; so that t_1 = T + 461°·2. Then

Energy in foot-pounds exerted by each pound of water

$$= 772\left\{\text{T} - 212° - 673°{\cdot}2 \text{ hyp. log.}\left(\frac{\text{T}+461°{\cdot}2}{613°{\cdot}2}\right)\right\}^{\dagger} \quad . \quad (3)$$

Final Volume of expansion of mixed water and steam, in cubic feet per pound,

$$= 18{\cdot}38 \text{ hyp. log.}\left(\frac{\text{T}+461{\cdot}2}{673°{\cdot}2}\right) \quad . \quad . \quad . \quad (4)$$

It is worthy of remark, *that the energy developed depends solely on the specific heat of the substance in the liquid state, and the initial and final temperatures*, and not on any other physical property of the substance.

The following table gives some results of the formulæ. The first column contains the temperature on the ordinary scale of Fahrenheit, with intervals of 36° Fahr. = 20° Centig. The second column contains the expansive energy of one pound of water, in foot-pounds. The third column contains the velocity, in feet per second, which that en-

* In the original paper the absolute zero of heat was assumed to be 272½ Centig. below the melting point of ice. The value now adopted, 274° Centig., is deduced from later experimental data.

† This agrees with the formulæ given by J. B. in the "Engineer," of the 2d. October, 1863, page 200.

ergy would impress on a projectile of the weight of the water itself: that is, one pound. The fourth column, the final volume of expansion of the water and steam, in cubic feet per pound. For convenience, a fifth column is added, containing the initial absolute or total pressures in pounds on the square inch. The last line of the table has reference to the case in which the water would be totally evaporated.

TABLE I.

Initial temperature.	Energy.	Velocity.	Final expansion.	Initial absolute pressure.
Fahr.	ft. lbs.	ft. per sec.	cubic ft.	lb. per sq. inch.
212°	0	0	0	14·70
248	726	214	0·95	28·83
284	2779	423	1·87	52·52
320	6052	624	2·73	89·86
356	10422	819	3·56	145·8
392	15826	1010	4·36	285·9
428	22156	1194	5·11	336·3
. .	. .	. .	. .	. .
about 2360	about 912500	about 7666	26·36	unknown.

In the absence of logarithmic tables, the following approximate formulæ may be used for temperatures not exceeding 428°.

$$\text{Energy, nearly} = \frac{772(T-212°)^2}{T+1134°\cdot4} \quad . \quad . \quad . \quad (5)$$

$$\text{Expansion, nearly} = \frac{36\cdot76(T-212°)}{T+1134°\cdot4} \quad . \quad . \quad (6)$$

Glasgow, October 5, 1863.

NOTE—*Added 19th October*, 1863.

In explanation of the formulæ and table it may be added, that the mechanical energy in column 2 is the equivalent of the heat which disappears during the process, being the difference between the whole heat expended and the latent heat of that portion of the water which at the end of the process is in the condition of steam at atmospheric pressure.

TABLE II.

Initial temperature.	Portion of the water which remains liquid.	Energy possessed by that liquid water.
Fahrenheit.	lb.	ft. lbs.
212°	1·000	0
248	0·964	700
284	0·931	2587
320	0·897	5429
356	0·865	9015
392	0·835	13215
428	0·806	17828
. .	. .	. .
about 2360	0	0

For the information of those who consider that the liquid portion of

the water, owing to small compressibility, acts like a volley of hard projectiles, a table (II.) is added, showing, for each of the initial temperatures in the previous table, what fraction of a pound of water continues in the liquid state, and how much of the energy developed is possessed by that liquid water.

In the formulæ and tables it has not been considered necessary to take into account the small increase which the specific heat of water undergoes as the temperature rises.

On the Behavior of Chloride, Bromide, and Iodide of Silver in Light, and the Theory of Photography. By HERMAN VOGEL.

From the London Chemical News, No. 205.

This paper contains the results of researches extending over three years, and undertaken by the author to determine the following questions:—

1. What effect has light on chloride, bromide, and iodide of silver?
2. What influence on the action have foreign substances, such as water, acids, free silver salt, and organic matters, &c.?
3. What changes do the silver compounds produced by light undergo in the developing process?

To answer the first question, what effect has light on chloride, bromide, and iodide of silver, many experiments have been made by other chemists, principally with the chloride of silver, some with the iodide, and very few with the bromide. The author briefly refers to these, which are of some importance in the history of photography.

Scheele was, perhaps, the first (1777) who studied the action of light on the chloride of silver. He noticed that it blackened in sunlight, and left metallic silver after treatment with NH_3; hence he concluded that light had decomposed the chloride into Ag and Cl.

A. Vogel and Wetzlar, however, found that the blackened AgCl could not be decolorized by heating with NO_5, nor was any silver dissolved, they therefore inferred that sunlight decomposed the chloride, into Cl and a subchloride. Wetzlar further remarked that the chloride blackened under nitric acid. Wittstein also observed that nitric acid dissolved no silver from blackened chloride.

Robert Hunt, on the contrary, maintains that metallic silver is formed on the surface of photographic paper together with Ag_2Cl and oxide of silver.

Guthrie and Draper affirm that AgCl is decomposed into Ag and Cl, and Guthrie supposes that the Ag is reduced to a passive state in which it is insoluble to NO_5. Draper says that the properties of the free silver are so changed that one might believe in the transmutation of the metal.

Spiller asserts that by a year's exposure to light the AgCl is changed into a body, the specific gravity of which favors the opinion that AgCl is decomposed into its constituents. Malone affirms the same, and supposes that a red and brown silver may exist.

Dawson, from the results of a long series of experiments, concluded

that the final action of light on AgCl was the formation of metallic silver, and that the reduction was gradual, the chloride passing through the stages AgCl, Ag_2Cl, Ag_3Cl, Ag_4Cl, &c.

Davanne and Girard assert the complete decomposition of the chloride.

Hardwich, on the contrary, contends for the formation of a subchloride.

The above opinions, and many others that might be quoted, show the different views on the nature of the change produced by light on chloride of silver.

With regard to bromide, Berthier says that this compound changes to a grey color, and blackens more slowly than the chloride.

Monckhoven asserts that it changes more rapidly than the chloride.

More extended researches have been made with the iodide by Daguerre and others. Moser and Draper agree that iodized silver plates undergo no chemical change on exposure. Schnauss supposes that iodide of silver is not chemically changed, but acquires the property of attracting particles of reduced silver "by a sort of electrical tension." He showed that no picture could be produced by iodide of silver alone, but that it was developed by a silver precipitate deposited on the exposed portions of the iodide. Later he conclusively proved that there are two modifications of the iodide—one sensitive the other non-sensitive. The first is produced when the iodide is precipitated in an excess of silver solution; the second when an excess of iodide of potassium is used. He states that the sensitive iodide becomes brown in the light, but seems to believe that this photographic property is owing to the presence of a trace of $AgONO_5$, which cannot be removed even by long washing. He thinks that the combination $AgI+AgONO_5$, which may be crystallized out of solutions containing AgI, is the really sensitive agent. He supposes, too, that a decomposition of the iodide into free iodine and a subiodide takes place on the long exposure of light.

Sutton asserts the non-sensitiveness of AgI prepared with an excess of KI, and is opposed to its chemical decomposition. Pure iodide he supposes to be insensible, and attributes the sensitiveness to the presence of a trace of nitrate. But he believes that the chloride and bromide are decomposed, the silver reduced, and Cl and Br set free. Davanne maintains that the iodide is decomposed into iodine and silver. Hardwich and Monckhoven have, on valid grounds, combated this view.

After this short review of the investigations of other chemists, the author proceeds to detail the results of his own *experiments with pure chloride, bromide, and iodide of silver.*

He prepared AgCl by gaslight, by precipitating $AgONO_5$ with HCl, and washing the precipitate until the washings gave no trace of Cl. AgBr and AgI were made by precipitating $AgONO_5$ with an excess of KBr and Kl. The chloride obtained as above was white,

the bromide was pale yellow, and the iodide straw-colored. After careful washing they were dried and kept in the dark.

To test their behavior towards light, small quantities were placed in test-tubes by lamplight, the salt was rubbed to a powder with a glass rod, and the tube was sealed or closed with a cork; the tubes were then exposed to light. A change of color was soon observed in the chloride and bromide; the former became violet, the latter grey. The color deepened with time, and reached a maximum in several days; the AgCl was then of a brownish violet color, and the bromide an impure pale greyish violet. The coloration was only superficial, the interior remaining white.

Iodide of silver prepared as above underwent no change of color in the course of some months exposure to intense light.

In most text-books it is said that AgCl and AgBr become black in the light. This is not correct; even after exposure for years they only become colored as above. The assertion of Seebeck that AgCl only blackens in the presence of moisture is equally incorrect. Perfectly dry and even fused chloride turns violet on exposure.

The change of color is always accompanied by a chemical change, in which chlorine and bromine are set free. Both of these I recognised by their odor, and by the reaction on iodide of potassium starch paper. A tube with AgCl and another with AgBr, having each a strip of paper fixed in the cork, were exposed to sunlight simultaneously. The paper in both instances became colored, in the case of the bromide very quickly, but with the chloride more slowly. The experiment repeated under the diffused light of a blue sky gave the same results. Experiments were also made with fused AgCl and AgBr. These were sealed in tubes; after several days exposure the tubes were opened, and a strip of the iodized paper introduced. The paper was only faintly colored by the chloride, but much more distinctly by the bromide. From these experiments we must conclude that by exposure to light AgCl and AgBr are decomposed, and chlorine and bromine set free, the decomposition taking place more quickly with the AgBr than with the AgCl.

The question now to be solved is what compound remains behind. As only pure chloride and bromide were exposed, and free bromine and chlorine were recognised, either (1) the compounds must have been decomposed into their constituents; or (2) a sub-chloride and sub-bromide must have been formed with the evolution of free chlorine and bromine. In the first case free silver must have been present in the compounds exposed. To detect this, chloride and bromide which had been exposed for months, were boiled in nitric acid (sp. gr. 1·2), and the clear liquor tested with dilute HCl; not a trace of silver was discovered in either case. From this it may be concluded that a sub-chloride and a sub-bromide are formed by exposure.

The objection of Guthrie that silver is reduced to a passive state is scarcely worth refutation, and the assertion of Hunt that oxide of silver is formed is contradicted by the above experiments. Spiller's

assertions are of too hypothetical a nature; he expresses conjectures which are only half supported by facts. The free silver obtained by Dawson, who exposed chloride of silver with free silver solution, or with solution of chloride of sodium, can be easily accounted for; nitrate of silver is decomposed in the light, and Wetzlar has shown that chloride of sodium has the power of decomposing Ag_2Cl with the separation of Ag.

The quantitative composition of the sub-chloride and sub-bromide of silver formed by exposure to light it is impossible to determine, since we are unable to separate the part affected by light from the unchanged AgCl and AgBr. The qualitative reactions, on the other hand, are of importance. Ammonia poured over deeply-colored AgCl dissolves the greater part, leaving only some grey granules. These, on pressure with a glass rod, become silver white, with a metallic lustre; and, as they dissolved when heated with NO_3, are metallic silver. The same reaction is observed in the case of the brown chloride of silver obtained by Wetzlar by the action of perchloride of iron on silver leaf, and also by Wohler by the action of HCl on protocitrate of silver. These bodies give no silver to nitric acid, but are decomposed by ammonia, chloride of silver being dissolved, and metallic silver remaining. The indifference towards nitric acid proves that these bodies cannot be regarded as mixtures of chloride and metallic silver.

That the compound obtained by exposure is really a mixture of Ag_2Cl and AgCl the author considers satifactorily proved by the fact that he obtained a body exhibiting precisely the same color and behavior towards nitric acid and ammonia on treating Rose's compound of ferroso-ferric oxide and protoxide of silver with hydrochloric acid. He prepared a similarly violet-colored chloride by treating blackened peroxide of silver (which he shows to be a mixture of AgO_2 and AgO) with hydrochloric acid, and this compound behaved toward nitric acid and ammonia in precisely the same way. Carbonate of silver, after exposure to light, also furnished a chloride containing Ag_2Cl, and the author thinks it extremely probable that all silver salts reducible by light are first reduced to a state of protoxide, or the analogous haloid salt, and that any metallic silver is the result of a secondary decomposition.

A sub-bromide, similar to the sub-chloride of silver, may be prepared by treating Rose's compound with dilute hydrobromic acid. A pale grey violet body is thus produced exactly like the body produced by light. On boiling with NO_5 it became grey, as happened with exposed AgBr. When NO_3 was poured over the mass boiled in NO_5 it took a darker grey color, and a grey powder settled at the bottom. This dissolved in NO_5, and gave a silver reaction. The exposed AgBr behaved in the same way.

We see therefore that the behavior of AgBr in light is exactly analogous to that of AgCl—that by exposure a body is produced which shows the same behavior towards reagents as the exposed AgCl—justifying the conclusion that it contains a sub-bromide

(Ag_2 Br). The decomposition with NH_3 appears to be more difficult in the case of sub-bromide than the sub-chloride, owing, most likely, to the more difficult solubility of the AgBr in ammonia.

I come now to the iodide of silver. I have already remarked that this suffers no *visible change* in the light. The question now is whether it undergoes any *chemical* alteration similar to that which takes place with chloride and bromide? I sought next to ascertain whether any iodine was set free. With this object I dropped freshly-prepared thin starch paste on AgI, which had been a long time exposed in a closed glass tube. No reaction followed. With AgI covered with starch paste and exposed to intense sunlight no reaction was observed, nor could free iodine be recognised with sulphide of carbon. I repeated this test with starch paste containing KI, but even with this was unable to discover any trace of free iodine. From this experiment I must come to the conclusion that by the exposure of AgI to light no iodine is set free. If therefore AgI suffers any decomposition it can only consist in the formation of free *silver* or a *subiodide*, and of a *superiodide*.

The information can only be ascertained in this by the solution of the undecomposed AgI in $NaOS_2O_2$; by this means I obtained *no residue* of free silver.

The formation of free silver in this case is also improbable, as it must have been recognised by the surface becoming grey. There remains therefore only the second supposition—the formation of a sub and superiodide. The formation of the latter, however, is very improbable. Notwithstanding their much stronger affinities Cl and Br have but a slight disposition to form superchlorides and superbromides, the disposition is still less in the case of iodine. We know, for example, perchloride of iron and perchloride of copper, but not periodide of iron and periodide of copper in a free state. In experiments on the formation of these bodies only iodide is produced, and iodine set free. I cannot therefore bring myself to believe in the formation of a superiodide of silver, the less so as it is very improbable that in case it existed it could withstand the action of iodide of potassium.

But if no superiodide is formed, and no iodine is set free, the formation of a subiodide or metallic silver is also impossible; the conclusion therefore is that on the exposure of AgI *no chemical decomposition takes place.*

I have hiherto described the behavior of the haloid silver salts prepared with an excess of the *precipitant* (HCl,KBr, KI). *The results obtained with haloid salts, prepared with an excess of silver salt, are somewhat different.* These salts were prepared by lamplight, and washed until the washings gave no silver reaction. I shall denote these salts AgClβAgBrβ and AgIβ, and those before described AgClαAgBα and AgIα. AgClβ and AgBrβ do not differ in external appearance from AgClα and AgBrα, but AgIβ is of a deeper yellow color than AgIα. More characteristic was the difference of the behavior of the salts in the light. AgClα *decomposes more slowly* in the light than AgBrα; AgClβ, on the contrary, *decomposes quicker*

than AgBrβ. I ascribe the quicker decomposition of the AgBrα compared with the AgBrβ to the finer division of the former.

With regard to coloration, AgClα and AgClβ, as well as AgBrα and AgBrβ, behave exactly alike. It is otherwise with AgIβ. *While AgIα does not show the least change of color in light, AgIβ becomes decidedly grey with a greenish tinge.*

On experimenting with AgIβ as with AgIα to ascertain whether iodine was set free by the exposure of the former, the author was brought to the conclusion that neither AgIα nor AgIβ underwent any change in the light.

(To be Continued.)

New Method of Engraving and Multiplying Prints, &c.

From the London Art Journal, January, 1864.

Of the many processes that have been proposed for expediting the production of engravings, none are so likely to attain the desired end as a method named after its ingenious inventor, the Proces Vial, many and various results of which are in the possession of Messrs. Hunt and Davies, in Searle Street, Lincoln's Inn Fields. After the admiration excited by a really simple and beautiful invention such as this, comes the question of its practical utility, and this is at once obvious. The prints we have seen from plates prepared by M. Vial's process, are in spirit and character so different, that they could not be pronounced as the results of one and the same method of re-production, some being marked "instantaneous," others, "five" or "ten" minutes, meaning that the design was, so to say, engraved, and the print produced, within a few minutes.

To begin with a fine line engraving. There is a small plate, of which the subject is a sportsman shooting a hare, in every respect like a proof from a plate engraved by a masterly hand: every blade of grass, every weedy tuft, is there, with the same precision that distinguishes the print taken from the plate in the usual way. Perhaps even clearer than this, is a print of a fawn in a thicket, which it is impossible to fancy other than a finished proof from a highly-wrought copper-plate. To artists, the most interesting capability of the invention is its power of transferring, in a few minutes, impressions of a drawing or sketch to a steel plate, line for line, touch for touch. M. Gerome, the celebrated French artist, made, in the presence of the French Commissioners of Fine Art, a sketch of the head of a dromedary, which was prepared and printed from in a few minutes. The sketch was drawn with lithographic chalk. There is by Jules David a sketch something between an etching and a wood-cut: the subject, a French cottage girl hearing a child read; also a dog by another artist, drawn with chalk and touched with India ink, wherein the spirit and manner of the drawing are so perfectly maintained, that it is difficult at first to determine it other than the drawing itself.

It will at once strike the experienced artist that it is by chemical agency the plate is prepared with such rapidity; and the means are so simple, that it is a matter of surprise it has not been before made subservient to the re-production of drawings, sketches, and all kinds of

designs to which the invention is applicable, for, from what we have said, it will be understood that the range it embraces is very wide. The preparation of the plate, and the impression taken from it, is, as we have said, effected in a few minutes. The inventor describes his process as "instantaneous;" in certain cases it is, and in all cases very rapid. An artist makes a drawing with lithographic chalk or ink on a steel plate; this is immediately immersed in a bath of a solution of copper, which leaves a thin coat of the metal on all exposed portions of the steel, but "bites in" the lines or touches left by a brush or crayon point. The action of the solution is so rapid, that experience is necessary to determine the point at which the corrosion must be arrested. In lithography a like means is used, but with a reverse result, inasmuch as the printing-surface remains in relief. Not only can the drawing be made directly on the steel and reproduced therefrom, but a drawing made on paper could be transferred to the steel without any alteration. We are told that the most elaborate engraving or lithograph can be reproduced on a steel plate in from ten to thirty minutes from a paper proof, without injury to the original. Thus it is impossible to say what limit can be set to the extensive utility of the invention. And now as to cost—that is said to admit of a reduction of sixty per cent. from the average expense of the production of engravings. It will be understood that in the multiplication of all fine works, as, for instance, engravings, the plates must first be engraved in the usual manner before they can be transferred; but as far as we have seen and understand the process, it must greatly reduce the cost of a large class of works for which the various and ordinary manners of engraving and printing are now employed.

Casting of the 20-inch Gun at Fort Pitt Foundry, Pittsburgh.

The 20-inch gun was cast at the Fort Pitt foundry, Pittsburgh, on Thursday noon. This immense gun, which was first suggested by Major Rodman in his report on the trial of the first 15-inch gun, has long been a matter of theory. Preparations for its manufacture were begun in May, 1863, and were continued from time to time as the exigencies of the service permitted.

The model is an enlargement of the first 15-inch gun. The length of the rough casting is 26 feet; its maximum diameter 66 inches; and its weight 180,000 pounds. The length of the finished gun will be 20 feet 3 inches; its maximum diameter 64 inches; and its weight 115,000 pounds. The diameter of the rough casting at the muzzle will be four feet, and that of the finished gun at the muzzle 34 inches. The whole length of the bore is 210 inches. The bore will terminate in a semi-ellipsoid, whose major axis will be thirty inches, and whose minor axis will be twenty inches.

The mould consists of a flask (made in four pieces, bolted and clamped firmly together), and of the sand which it contains, which forms the matrix of the gun. The flask weighs twenty-eight tons, and the sand ten. The mould is made by placing the flask over a wooden pat-

tern, having the form of the rough casting, and then ramming the sand between the pattern and the flask. The pattern is then withdrawn, the mould is "slicked" or smoothed over, and then washed with a black coating made of ground coal. The mould is then placed in an oven, and baked until it has the hardness of an ordinary soft red brick; this mass of sand was rammed, finished, blacked, and placed in the oven, by ten men, in twenty hours. The flask being removed from its ovens, and clamped firmly together, is placed vertically in a pit, made for its reception. Thus disposed, the muzzle end of the mould is on a level with the mouth of the pit, and the centre line of the gun is perpendicular.

The pit which receives the flask is twenty-eight and a half feet deep, and fourteen feet in diameter. Inside it, near the bottom, grate bars radiate like the spokes of a wheel. Underneath on the outside, these flues communicate a draft. Another flue, leading from the top of the pit, carries off the smoke. The object is to maintain a strong fire around the flask. This is one of the precautions in the process of casting guns.

The core is a long, hollow, cast iron, fluted barrel, hung vertically in the centre of the mould. It is exteriorly coated—firstly, by a coil of three-eighths inch rope, closely wrapped, and a mile in length; and, secondly, with a luting of stiff clay, which is put on to separate the cast iron of the barrel from the molten iron of the gun. The rope is used to keep the clay out of the grooves, and, by presenting a rough surface, to prevent it slipping off. The grooves allow the gas (mainly hydrogen), formed by the contact of the melted metal and the clay, to escape freely at the upper end into the air, where it burns with quite a large flame. After the coatings are on, the core is likewise baked hard in an oven. Its outside diameter is nineteen inches, one inch less than the finished diameter of the bore, thus allowing half an inch all round for the completion of the latter.

The core barrel is kept cool by means of a stream of cold water circulating throughout its whole length. A small pipe extending almost to the bottom of the core is traversed by the water which, rising, fills the barrel, and passes off at the top. During the casting, the flow of water was sixty gallons per minute.

Three large reverberatory or air furnaces are used. Two are of the capacity of twenty-five tons each. The third, of the capacity of forty tons, is believed to be the largest in the world. It was built expressly for this casting. It was charged with thirty-nine tons, and each of the smaller furnaces was charged with twenty-three and a half tons. From these furnaces the iron is conducted through iron troughs, lined with clay, into a common pool near the pit. Thence it is conducted by two troughs into the gates of the mould. These gates are two openings, three inches in diameter, extending all the way down the mould outside the matrix of the gun. They communicate with the gun proper by means of smaller gates, cut through at intervals of fifteen inches, all the way up. The iron is thus conducted first into the bottom of the mould, and then through the side-gates, respectively, up to the top, as

the metal rises in the mould. The difference between the reverberatory and the cupola furnace is that in the former, the fuel and iron are separate, and there is a natural draft through a large stack. In the latter, the fuel and iron are together, and the draft is made by blast.

Two cranes are used in lifting this heavy mould, and in taking the gun from the pit. They have each the capacity of forty tons. They are worked by the steam engine, which not only hoists and lowers the load, but likewise causes the cranes to revolve.

The metal from which the gun is manufactured is the Juniata iron, from the Bloomfield-Juniata and Rodman furnaces.

It has been once re-melted before being charged into furnaces for the gun. The iron from which it is constructed is thus all of one grade.

The furnaces were fired at five o'clock, A. M.; the metal was all melted by eleven o'clock. Whilst the iron was running in the mould, it was constantly stirred by men with long poles of oak. The object of this was to relieve it of the gas often formed in gun-irons, and which, if not removed, creates globular cavities in the mass of the gun, and thereby destroys its uniformity.

The casting commenced at twenty-four minutes after twelve. Eighteen bushels of coal were allowed to each ton of iron to make the fusion. Furnace number five was first opened, and then furnaces numbers six and four; four men with long, narrow puddling sticks, collected the scum and scoriæ which gathered on the surface of the liquid iron. Small quantities of iron where thrown into the pit from time to time, in order to light the gas which escaped from the sand through the small vent-holes in the flask.

The gas escaping from within the iron pipe surrounding the core barrel, was in a constant state of ignition, and formed a pale, luminous crown of blue flame; the last furnace was closed at forty-five and a half minutes past twelve, and the time occupied in the casting was twenty one and a half minutes.

The temperature of the stream of water was at the commencement, thirty-six degrees; at the moment the casting was over, it stood at forty-two degrees; four minutes after the casting was over it was at fifty-two degrees; eight minutes after the casting was over, at seventy-five and a half degrees; fourteen minutes after the casting, at eighty-one and a half degrees; twenty minutes after the casting, at eighty-nine and a quarter degrees; twenty-five minutes after the casting, at ninety-one degrees; and thirty minutes after the casting, at ninety-one and three-quarter degrees.

Nothing went wrong. Furnace No. 6 was the first to be stopped, and the others, Nos. 4 and 5, were stopped almost immediately after. When the casting was completed, a blacking, consisting of ground coal, was strewn over the top to prevent chilling. During the operation the gas escaped freely from the vent-holes in the flask, which is perforated in a thousand places, half an inch in diameter. Three furnaces only were employed. A fourth furnace was in reserve, in case of any accident, but there was no necessity for its use. Only two or three tons were left over after the casting was completed.

The fourth furnace in reserve had a supply of ten tons, and a fifth twelve tons, which in any exigency might have been drawn upon. After the casting was over, the mould, not being completely filled, owing to the gradual settling of the liquid mass, was supplied with fresh liquid iron, or, in other words, the workmen "filled up the shanks." All the fuel employed was the Pittsburgh bituminous coal.

The drawing of the core was to take place about twenty-four hours after the casting. In this operation the supply of water is suddenly stopped off. The barrel is much expanded by heat. A large strain is put on by the steam crane, almost sufficient to draw the barrel out on the re-admission of water. The barrel cools quickly, shrinks with rapidity, the strain of the crane springs it out about two feet, and it is then drawn out.

The buildings, cranes, furnaces, patterns, flask, and lathe, had to be entirely new-constructed in order to accommodate themselves to the size of the gun.

The lathe is sixty feet long, eight feet wide over the shears, and is driven by two engines, 6-inch cylinders, 12-inch stroke. In the foundation there are one hundred and twenty thousand bricks. The costly nature of the gun may be appreciated when it is remembered that the lathe which merely turns and finishes it cost between ten and fifteen thousand dollars.

The gun will be hung on trunnions placed over its centre of gravity, and allowing no preponderance. This mode of hanging guns, renders their manipulation in service much easier than when a great preponderance is at the breech.

The gun carriage does not deviate in its general construction from the wrought iron carriages of the 15-inch guns. The carriage is now being manufactured at the Watertown Arsenal, near Boston.

No statement can at present be given respecting the tenacity and hardness of the metal. It is to be remarked, that by the interior cooling principle adopted, the hardest and best iron is that nearest the surface of the bore. The fact of the gun being cast hollow, secures this.

The iron of which the gun is cast was thoroughly tested, in the same manner as that of the first 15-inch. The sample by which the gun is to be proved is to be taken from the sinking-head, or the rough end cut off from the muzzle.

Special cars or trucks are being built by the Pennsylvania Railroad Company for the proper conveyance of the gun. Its great weight concentrated in a small space might prove destructive to ordinary cars. A very large truck is therefore rendered necessary. The length of the gun is thus distributed.

The new proving ground belonging to the Fort Pitt Foundry, and known as Fort Pitt station, is seventeen miles out of the city, on the line of the Pennsylvania Railroad. A large traversing train, propelled by steam, has there been erected to handle the gun. Two heavy triangular frames, about forty feet high, support a movable beam on a steel knife-edge bearing, from which heavy rods descend and catch the trunnions and breech of the gun. Suspended in these rods the

gun will be three feet from the ground, and after a firing will vibrate a pendulum between the frames. The shot discharged will be thrown into a steep but soft rock.

The powder used here is $\frac{1}{8}$ of an inch in diameter, or about the size of hickory-nuts. The gun will burn not less than one hundred pounds of powder.

The 20-inch is not intended for a long-range gun, but rather to do execution within the range which it will possess. In the manufacture of guns, of large calibre, the question which arises is, "to which shall the greatest attention be devoted, to the increase of velocity, or to the bulk of the cannon ball?" The weight of the shot has in this instance engrossed consideration. Shell will be almost exclusively used. Both shot and shell will be spherical, of great weight and enormous explosive force. The solid shot will weigh one thousand pounds, and the shell seven hundred and seventy-five pounds, allowing for windage. No information of course can be given at present in regard to the range of the gun.

The value is estimated at thirty thousand dollars. The gun is expected to be ready in the latter part of May. The expense for each charge of the gun may be roughly placed at about seventy-five dollars, allowing twenty-five dollars for powder, and fifty for projectiles. Fifteen or sixteen men will be required to manage the gun when completed, nine being employed in the loading, and six or seven in traversing the gun.

The casting was then taken from the pit on the 25th of February, having been permitted gradually to cool until that time, and was then found apparently perfect.

Oxygen Gas.

From the London Chemical News, No. 212.

Dr. Frankland stated that he had lately been engaged in some experiments wherein he was desirous of using oxygen gas under great pressure, and for this purpose had procured the apparatus for the compression of gases made by Natterer, of Vienna. During the employment of this apparatus an accident happened, which fortunately occasioned no serious disaster, but was the means of indicating a result which was not likely to have been anticipated, and which he considered well worthy the attention of the Society. The apparatus consisted of two parts. On the one hand, a powerful compression pump, worked by a crank and fly-wheel, which forced the oxygen or other gas into a strong cast iron receiver, fitted with a conical screw-plug and other joints and connexions of steel. The action of the pump had been maintained until twenty-five atmospheres of oxygen had been accumulated within the receiver, when suddenly this latter part of the apparatus exploded, at the same time diffusing a shower of sparks, reminding one of the phenomenon observed when iron or watch-spring was burnt in oxygen. On examining the shattered fragments of the iron receiver, it was manifest that this result had indeed happened; and it appeared that the combustion of iron was possible under the circumstances, a small

quantity of oil used as a lubricant becoming, no doubt, in the first instance, ignited, and then imparting the combustion to the iron. The whole interior surface of the iron receiver was blistered and coated with fused globules of magnetic oxide of iron, the small tubular apertures through the screw-plug were widened to four-fold dimensions, and no less than half an inch of steel was burnt off the massive head of the apparatus. It was, perhaps, so far fortunate, that a defective joint rendered it impossible to obtain more than the pressure of twenty-five atmospheres, under which the apparatus exploded, for had it been augmented to forty atmospheres, the iron must have taken fire more readily, and perhaps have been completely consumed or hurled about as red-hot bolts, endangering both life and property. The author had, by a rough calculation, arrived at the conclusion, that the heat developed by the union of half the contained oxygen with the iron, would have been sufficient to melt the cylinder, which would then have exploded by the remaining pressure of the other half of the gas. Dr. Frankland concluded by referring to the possibility of applying this principle in the construction of shells and other implements of warfare.

In answer to an inquiry on the part of the President, Dr. Frankland stated that the degree of pressure in the apparatus was estimated, not by a gauge, but by actual measurement of the oxygen gas passing in at the ordinary temperature and pressure from a pneumatic holder. It was right also to mention the fact of water and potash being likewise contained in the iron reservoir at the moment of the explosion.

Dr. W. A. Miller once met with an accident in his laboratory at King's College, which, although totally different in character from that just before narrated by Dr. Frankland, should be made known by way of warning chemists from attempting a like experiment. The speaker had been using a powerful Rühmkorff coil for the purpose of effecting the combination of the mixed oxygen and hydrogen gases in Cavendish's eudiometre, and when the first charge had thus been disposed of, the stop-cocks were opened for the admission of a fresh supply of the mixed gases. At this moment, whilst yet disconnected with the electric coil, the whole arrangement exploded, and, so far as the instantaneous character of the phenomenon could be observed, it appeared to be due to the formation of a fine deposit of spongy platinum upon the metallic wires inserted into the eudiometre, the result of the employment of an electric instrument of too great intensity. By reverting to the old method of causing the ignition by the spark from a Leyden jar, no difficulty or danger had ever been experienced.

A Substitute for Wood Engraving.

From the London Art Journal, Nov., 1863.

Multifarious have been the attempts, throughout the latter ages of the world especially, to bring scientific knowledge to bear upon the productions of human industry, in order to lessen, if not entirely to

supersede, the efforts of manual labor; and so far as the operations have been directed to the machinery of construction and to manufacturing processes, the results have proved marvellous. It seems as if no limit could be assigned to the ingenuity of man in devising and creating what would best minister to his absolute necessities in all instances, and to his gratifications in not a few. Similar successes have not, however, followed his endeavors when he has ventured into the region of Art; here matter will not yield obedience to mind, so to speak; though photography and chromo-lithography, and other methods of artistic production, have done something in the way of superseding the hand-work of the painter, and machinery is employed to do that which, a few years ago, was accomplished only by the tools of the sculptor. Art, as a creator and skilled workman, defies all scientific aid; it is, and must be, self-dependent.

Perhaps there has been no other department of Art in which so many efforts have been made to find a substitute for actual labor as wood-engraving; hitherto, however, all have failed, and the engravers are still left in quiet possession of the field; nor are we by any means sure that what we now lay before our reader will be the means, even ultimately, of dislodging them from the position they have so long and so honorably held in the domain of Art. But it is our business to inquire into whatever comes before us in the way of novelty which bears on the face of it a probability of success, while it is a duty to any one who seems to have made out a case to give it all the publicity in our power. For this purpose we introduce an account of Mr. Schulze's process of producing blocks for type-printing, intended to do away with wood engraving.

Mr. Schulze is a German architect, resident in New York, but now staying in London for a short time. He informs us that the material on which the drawing is to be made may be of glass, or any other hard and smooth surface. The drawing is produced with a pen, and ink composed of pure gum arabic dissolved in water, with the addition of sufficient sugar to prevent it cracking when dry; lamp-black, or any other color, is mixed with the gum solution to render the work visible. When the drawing is completed, it is covered with a coat of bees-wax, asphaltum, resin, and linseed oil. The thickness of the covering depends on the kind of work adopted by the artist; if the lines of the drawing are very close together, a thin coat will suffice. After this ground has been applied, the plate, or glass, has to be submerged in water for about ten or fifteen minutes; then a strong stream of water is poured upon it, which will remove the waxy substance *above* the lines of the drawing, but will leave that *between* the lines undisturbed. In most cases the grounding will be sufficiently high to insure a good electrotype for printing; but where considerable height is required between lines far apart, this can readily be effected by applying wax according to the method now employed by stereotypists, or by adding asphaltum with the brush. Should the artist prefer to make his drawing on paper, the latter must be rendered waterproof; and after it has undergone this process, it should be attached, with a waterproof

paste, to a hard and even plate before it is covered with the wax; in all other respects it is treated in the manner just described. Before taking the electro deposit, the plate must be covered with a thin coat of alcoholic varnish, and when dry, black lead—plumbago—is applied with a soft brush.

The advantages of the process are stated by the inventor to be:—The obtaining a perfect fac-simile of the artist's work; the drawing has not to be reversed, as in the methods now in use for copying on the wood pictures or objects; cheapness, and saving of time.

FRANKLIN INSTITUTE.

Proceedings of the Stated Monthly Meeting, January 21, 1864.

John C. Cresson, President, in the chair.

John F. Frazer, Treasurer.

Robert Briggs, Recording Secretary, pro tem.

The minutes of the last meeting were read and approved.

A letter was read from Messrs. Eives and Macey, London.

Donations to the Library were received from the Royal Astronomical Society, the Institute of Actuaries, and the Society of Arts, London; de Oesterriechischen Ingenieur-Veriens, Vienna, Austria; Professor E. M. Dingler, Augsburg, Germany; the Natural History Society, and Major L. A. Huguet-Latour, Montreal, and the University of Toronto, Canada; the Hon. Commissioner of Agriculture, the Hon. Wm. D. Kelley, Prof. A. Dallas Bache, and Frederiek Emmerick, Esq., Washington, D. C.; the Hon. Mr. Brady, Senate, Harrisburg, Penna.; and Prof. John F. Frazer, Philadelphia.

The Periodicals received in exchange for the Journal of the Institute, were laid on the table.

The Treasurer read his statement for December, 1863, also his annual statement for the year 1863.

The annual statement of the Committee on Publications on the state of the Journal of the Institute, for 1863, was read.

The Board of Managers and Standing Committees reported their minutes.

Candidates for membership in the Institute (16) were proposed, and the candidates proposed at the last meeting (12) were duly elected.

The Tellers of the Annual Election for Officers, Managers, and Auditors, for the ensuing year, reported the result, when the President declared the following gentlemen duly elected:—

William Sellers, President.

John H. Towne, Fairman Rogers, } Vice Presidents.

Washington Jones, Recording Secretary.

Robert Briggs, Corresponding Secretary.

John F. Frazer, Treasurer.

MANAGERS.

William Adamson,	John Gardiner, Jr.	Charles T. Parry,
B. Henry Bartol,	George Harding,	Percival Roberts,
Charles H. Cramp,	James Hunter,	Samuel J. Reeves,
Pliny E. Chase,	W. Barnet Le Van,	Coleman Sellers,
Charles S. Close,	J. Vaughan Merrick,	Samuel Sloan,
William Divine,	Wm. A. Mitchell,	Samuel S. White,
James Dougherty,	Bloomfield H. Moore,	S. Lloyd Wiegand,
George Erety,	Henry G. Morris,	O. Howard Wilson.

AUDITORS.

Samuel Mason, James H. Cresson,
William Biddle.

At a meeting of the Board of Managers, held January 27th, 1864, the following officers were elected for the ensuing year:—

James Dougherty, Chairman.
Bloomfield H. Moore, } Curators.
W. Barnet Le Van, }

On motion of Coleman Sellers, the thanks of the Franklin Institute was tendered to Professor Cresson for the able and efficient manner in which he has filled the office of President, the past *nine* years.

Mr. Briggs remarked that is was eminently proper, that upon retiring from the office, as Professor Cresson does this evening, the thanks of the Institute be offered him individually, as a mark of the appreciation of the ability and attention with which he had for so many years so honorably filled the highest seat in its councils.

The Institute represented not only the relation between the mechanic and the scientific man in Philadelphia, but was beyond that, the representative institution of the connexion of mechanics with science for the country. No similar institution had its reputation either at home or abroad, and that position has been reached by those who cotemporaneously with Professor Cresson gave it an original direction, and it has been supported in that position the last nine years, under his leadership. To no one more than to our retiring President, do we owe the elevation of the Institute, and on this occasion, when new and inexperienced hands have been entrusted with its direction, we can only mark our estimation of his example by a vote of thanks.

Mr. Cresson replied, that he highly appreciated the personal kindness that prompted the gentleman to move this resolution, and the kindness of which its adoption by the Institute gave evidence. He must, however, disclaim the credit of having contributed in any large degree to the honorable position of the Institute as a pioneer in the noble work of raising the labor of the mechanic and artisan to its true position as both handmaid and exemplar of science. He was not one of the original members or founders of the Institute, having been absent from his native city during the years in which it had its inception; but united himself with it immediately upon his return to the city, and has for more than twenty-five years been happy to take a share in its labors.

For ten years he filled the chair of Mechanics and Natural Philosophy, in the lecture room; for about twenty years has acted as chairman of the Committee on Science and the Arts; and during the last nine years has been honored with the office of President. For these repeated marks of confidence, he is happy to express his grateful appreciation; and trusts that the Institution under the impulse of the younger and more active members who are now to guide its destiny, will put forth its strength in a long career of increasing usefulness.

On motion of Prof. Cresson, the thanks of the Institute were tendered to the retiring Vice Presidents, Secretaries and Managers for the able support and direction of the Institute during many years of official connexion.

Mr. P. E. Chase gave an abstract of his recent communications to the American Philosophical Society, on the barometer, as an indicator of the earth's rotation, and the sun's distance.

The existence of daily barometric tides, has been known for more than a hundred and fifty years, but their cause is still a matter of dispute. It is evident that they cannot be accounted for by variations of temperature, for, 1, their regularity is not perceived until all the *known* effects of temperature have been eliminated; 2, they occur in all climates, and at all seasons; 3, opposite effects are produced at different times, under the same average temperature. Thus at St. Helena, the mean of three years' hourly observations, gives the following average barometric heights:

From 0h. to 12h. 28·2801 in.	From 18h. to 6h. 28·2838 in.
" 12h. to 0h. 28·2861 "	" 6h. to 18h. 28·2784 in.

The upper lines evidently embrace the coolest parts of the day, and the lower lines the warmest. Dividing the day in the first method, the barometer is highest when the thermometer is highest; but in the second division the high barometer prevails during the coolest half of the day.

On account of the combined effects of the earth's rotation and revolution, each particle of air has a velocity in the direction of its orbit, varying at the equator, from about 65,000 miles per hour, at noon, to 67,000 miles per hour, at midnight. The force of rotation may be readily compared with that of gravity, by observing the effects produced by each in twenty-four hours, the interval that elapses between two successive returns of any point, to the same relative position with the sun. The force of rotation producing a daily motion of 24,895 miles, and the force of terrestrial gravity a motion of 22,738,900 miles, the ratio of the former to the latter is $\frac{24895}{22738900}$, or ·00109. This ratio represents the proportionate elevation or depression of the barometer above or below its mean height, that should be caused by the earth's rotation, and it corresponds very nearly with the actual disturbance at stations near the equator.

From 0h. to 6h. the air has a forward motion greater than that of the earth, so that it tends to fly away; its pressure is therefore diminished, and the mercury falls. From 6h. to 12h. the earth's motion is

greatest; it therefore presses against the lagging air, and the barometer rises. From 12h. to 18h. the earth moves away from the air, and the barometer falls; while from 18h. to 24h. the increasing velocity of the air urges it against the earth, and the barometer rises.

If the force of rotation at each instant be resolved into two components, one in the direction of the radius vector, and the other parallel to the earth's orbit, it will be readily perceived that whenever the latter tends to increase the aerial pressure, the former tends to diminish it, and vice versa. Let B=the height of the barometer at any given instant; M=the mean height at the place of observation; $\theta - 90°$= the hour angle; C=the earth's circumference at the equator; t=24 hours; g= the terrestrial gravity; l=the latitude; and a simple integration gives the theoretical formula, $B = M\left(1 + \frac{\text{sin. } \theta . \text{ cos. } \theta . \text{ cos. } l}{R^3} . \quad \frac{2c}{gt^2}\right)$*.

This formula gives a maximum height at 9h. and 21h. and a minimum at 3h. and 15h. The St. Helena observations place the maximum at 10h. and 22h., and the minimum at 4h. and 16h.—an hour later in each instance, than the theoretical time. This is the precise amount of retardation caused by the inertia of the mercury, as indicated by the comparisons with the water barometer of the Royal Society of London.

Aerial currents, variations of temperature, moisture, and centrifugal force, solar and lunar attraction, the obliquity of the ecliptic, and various other disturbing causes, produce, as might be naturally expected, great differences between the results of theory and observation. But by taking the grand mean of a series of observations, sufficiently extended to balance and eliminate the principal opposing inequalities, the two results present a wonderful coincidence.

According to our formula, the differences of altitude at 1, 2, and 3 hours from the mean, should be in the respective ratios of ·5, ·866, and 1. The actual differences, according to the mean of the St. Helena observations, are as follows.—

	Differences of Barometer.			Ratios.		
Diff. of time	1h.	2h.	3h.	1h.	2h.	3h.
Before 1h,	·0166	·0298	·0365	·455	·816	1
After "	·0159	·0266	·0298	·534	·893	1
Before 7h.	·0122	·0202	·0243	·502	·831	1
After "	·0135	·0239	·0297	·455	·805	1
Before 13h.	·0136	·0248	·0284	·479	·873	1
After "	·0131	·0215	·0227	·577	·947	1
Before 19h.	.0161	·0287	·0348	·463	·825	1
After "	·0150	·0265	·0286	·524	·927	1
MEAN	·0145	·0252	·0293	·495	·860	1

The mean of the above differences, varies from the theoretical mean, less than $\frac{1}{500}$ of an inch. If we take the mean of the ratios, instead

* $\frac{c}{gt^2}$ represents the effective ratio of an entire day. But there is in each day a half day of acceleration, and a half day of retardation, and the ratio for each half day is $\frac{c}{2} \div \frac{gt^2}{4} = \frac{2c}{gt^2}$.

of the ratios of the means of the observed differences, the coincidence is still more striking.

Difference of Time,	1h.	2h.	3h.
Means of Observed ratios,	·498625	·864625	1·000000
Theoretical Means,	·500000	·866025	1·000000

The calculated time for the above observed means, differs less than 20″ from the actual time.

Observed Means,	·498625	·864625	1·000000
Theoret. Diff. of Time,	59′ 48″	119′ 40″	180′
Observed " " "	60′ 0″	120′ 0″	180′

The varying centrifugal force to which the earth is subjected by the ellipticity of its orbit, must, in like manner, produce annual tides. The disturbing elements render it impossible to determine the average monthly height of the barometer with any degree of accuracy, from any observations that have hitherto been made. We may, however, make an interesting approximation to the annual range,—still using the St. Helena records, which are the most complete that have yet been published for any station near the equator. Comparing the mean daily range, as determined by the average of the observations at each hour, with the mean yearly range, as determined by the monthly averages, we obtain the following results:

Year.	Daily Range.	Annual Range.	Ratio.	Approximate Solar Distance.
1844	·0672 in.	·1650 in.	2·4553	137,070,000 m.
1845	·0646 "	·1214 "	1·8793	80,300,000 "
1846	·0670 "	·1214 "	1·8120	74,650,000 "
	3)·1988	3)·4078	3)6·1466	
	·0663	·1359	2·0489	95,446,000 "
Mean	·0663	·1290	1·9457	86,056,000 "
	2)·1326	2)·2649	2)3·9946	
	·0663	·1324	1·9973	90,702,000 "

The approximate estimates of the solar distance, are based on the following hypothesis:

Let e = effective ratio of daily rotation to gravity.

a = arc described by force of rotation in a given time t.

r = radius of relative sphere of attraction, or distance through which a body would fall by gravity, during the disturbance of its equilibrium by rotation.

A = area described by radius vector in time t.

Let e', a', r', A′, represent corresponding elements of the annual revolution. Then,

$$\text{A} : \text{A}' :: ar : a'r' :: e^2 : e'^2$$

But the forces of rotation and revolution are so connected, that a differs but slightly from a'

$$\left.\begin{array}{l} e^2 : e'^2 :: r : r' \\ r' = \dfrac{e'^2 r}{e^2} \end{array}\right\} \text{very nearly.}$$

$\therefore$

It may be interesting to observe how nearly r (22,738,900 m.) corresponds with Kirkwood's value of $\frac{D}{2}$ (24,932,000 m.). A more thorough comprehension of all the various effects of gravity and rotation on the atmosphere, would probably lead to modifications of our formulæ that would show a still closer correspondence.

There is a great discrepancy between the determinations of the solar distance that are based on the records of 1844 and 1846, but it is no greater than we might reasonably have anticipated. On the other hand, it could hardly have been expected that any comparisons based on the observations of so short a period as three years, would have furnished so near an approximation to the most recent and most accurate determination of the earth's mean radius vector. In order to obtain that approximation, it will be seen that I took, 1st, the mean of the ranges and ratios for the three successive years; 2d, the ranges and ratios of the mean results of the three years; 3d, the grand mean of these two primary means. I could think of no other method which would be so likely to destroy the effects of changing seasons, and other accidental disturbances.

The following table exhibits the effects of latitude on the aerobaric tides. The differences between the theoretical and observed ranges, may be owing partly to the equatorial-polar currents, and partly to insufficient observations.

Station.	Lat.	Mean height.	Mean Range.	Ratio.	Theoret. Ratio.
Arctic Ocean,	78° 37′	29·739 in.	·012 in.	·000404	·000527
Girard College,	39 58	29·938	·060	·002004	·002046
Washington,	38 53	30·020	·062	·002065	·002079
St. Helena,	15 57	28·282	·066	·002344	·002567
Equator,	0	30·709	·082	·002670	·002670

The theoretical ratios are determined by multiplying the equatorial ratios by $\frac{\cos l}{R}$. The formula, $\rho = \frac{\cos l}{R} \cdot \frac{2c}{gt^2}$, ($\rho$ indicating the ratio of the mean range to the mean height,) gives

		Theoretical Ratio.	Observed Ratio.
Latitude,	0°	·002190	·002670
"	78 37′	·000432	·000404

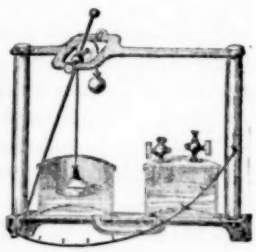
showing that the ratio is less near the pole and greater near the equator, than our theory indicates, a natural consequence of the centrifugal force at the equator, and the cold surface currents that produce the trade winds.

The revolution of the sun around the great Central Sun must also cause barometric fluctuations that may possibly be measured by delicate instruments and long and patient observation. The Toricellian

column may thus become a valuable auxiliary in verifying or rectifying our estimates of the distances and masses of the principal heavenly bodies.

Mr. Robert Briggs called the attention of the meeting to an instrument for measuring small pressures of air, such for example as were formed in ventilating air ducts, either when fans were used for impelling the air, or when self-acting currents were produced by heat; or in short, the amount of pressure which any current of air might possess. This instrument consisted of two cylindrical cups made of exactly the same diameter, the size being immaterial, but the exact correspondence of diameter was readily produced by the workmen using the same length of sheet metal in forming the two cylinders. One of these cups is closed except a cock to receive the pressure of air through a flexible tube which shall connect the instrument with the duct in which the pressure is to be measured; an air cock for setting the index, and a tubular connexion at the bottom with the other cup.

The other cup, connected to the first by a tube in the bottom, is open to the air. The open cup has a float depending from a cord which passes over a small grooved pulley, and terminates in a weight. The pulley is fast upon the shaft which carries the hand. This shaft is very lightly hung on centres, and the hand is carefully balanced, so that the friction of the cord on the pulley will be sufficient to move the hand. Attached to the side of the cups is a dial segment against which the extremity of the hand marks the pressure, in the instrument drawn in the cut. The cups were three inches diameter and two inches deep; the hand was eight inches long and the quadrant ten inches on the curve. The ten inches of motion indicating one inch of water pressure.



To operate the instrument the cups are filled with water three-quarters of an inch to an inch deep, and the end of the finger placed at zero by sliding the cord on the pulley when the pressure cock is closed and the air cock open. By opening the pressure cock and closing the air cock, the water is depressed in the first cup and elevated in the second, raising the float and thus transferring the movement greatly increased to the hand. By agitating the instrument so as to wet the sides of both cups before making the trial, correct indications of one-hundredth of an inch water column are readily attained. In order to obtain the mark on the quadrant originally without calculating the exact proportion of pulley to length of hand, the thickness of cord, &c., it is only necessary to thrust a needle through the cord, and then after setting the hand at zero, produce such a pressure as will move the needle (the point of which can rest on a slip of card) a half inch; the movement of the hand on the quadrant then indicates

one inch of difference of level in the two cups, and the extreme length thus ascertained can be divided to any desirable extent. For more feeble pressures the ratio of radius of pulley to length of hand can be increased by reduction of pulley, thus producing more motion of extremity of hand for a given pressure. There are similar instruments for considerable pressures, say one to six inch water column, but the speaker thought none, not requiring nice calculations as to areas of float chamber compared with water chamber, as well as the adjustment of pulley and length of hand, all of which ended in comparison with other instruments, for a basis of pressure measure, but he had not met with one before, which could be adjusted by itself, and was equally portable.

Professor Cresson described a Pressure Indicator used at the City Gas Works, for showing the pressure of the gas in various parts of the factory, and in the street mains, with all of which it is connected by small tubes provided with stop-cocks, that allow any one main to be in communication with the instrument, while all the others are cut off. These connecting tubes open into the upper part of a gas-tight receiver kept partly filled with water, or other liquid. An open stand pipe of small sectional area in comparison with the area of the receiver, passes through the top of the instrument, and descends low enough to have its lower end always immersed in the liquid, and thus sealed against all escape of the gas which occupies the upper portion of the receiver. The change of level of the liquid column in the stand pipe gives motion to a float, and this is communicated to a revolving index by a weighted cord running over a small pulley fixed on the arbor of the index.

The indications of pressure by the rise of the liquid in the stand pipe, are magnified in a ratio determined by the relative diameters of the small pulley that moves the index and of the graduated arc traversed by the latter.

If great accuracy is required, the minute error arising from the small changes of level of the liquid in the receiver as portions of it pass into, and out of, the small stand pipe, may be corrected in the divisions upon the graduated arc. For example, if the relative area of the receiver and the dip pipe, be 50 to 1, then the divisions should be one-fiftieth smaller than their designation indicates.

A stop-cock placed on the top of the receiver to admit atmospheric pressure, allowed the instrument to be brought to zero after each experiment.

Prof. Cresson also gave an account of experiments upon the momentum of air or gas flowing in closed pipes of large diameter. In a 20-inch gas main three miles long, he had found the momentum of the column to produce an excess of pressure, on closing a valve at one extremity, equal to a six-inch column of water, and the successive reflections of the wave to create nearly this excess of pressure alternately at the opposite ends of the pipes for many successive oscillations of pressure in the gaseous column.

Mr. Skerving exhibited a large Engraving by John Sartain, (from the original painting by C. Schussele,) entitled "Men of Progress." It represents a group of nineteen of the prominent American inventors and discoverers.

The names of the artists are sufficient guarantees of the excellence of the work, which is a well deserved compliment to those whose exertions have contributed so much towards the advancement of science and the arts.

Mr. J. T. Kerby exhibited a patent Horse-shoe. This is said to be the only shoe in use which is beveled on the inside so as to permit the dirt to readily detach itself from the hoof. There are seven instead of three "calks," as in the ordinary shoe, and consequently that even bearing all around the wall or crust, which the old shoe takes away, is secured.

Mr. Hiram A. Kimball exhibited his patented Artificial Limbs, made of vulcanized india rubber. They are light in weight, well shaped, and exceedingly strong. Being hollow, all the machinery is contained inside, and is not liable to be deranged or broken. These limbs can be much more readily manufactured, and in less time, than those carved from wood, or made of iron, as in the ordinary manner.

Mr. Thomas Shaw sent to the meeting, as a donation to the Institute, a very handsome figure of an individual standing on a trunk, and exerting all his strength in endeavoring to raise himself and the lid on which he was standing by pulling at the handles attached to it.

In the note, accompanying the figure, Mr. Shaw says—"I determined upon giving a short spicy illustration of a whole lecture on the subject, and to illustrate the folly of ever attempting to overcome that one *dead point* which attends all perpetual motion schemes."

A number of specimens of Wood Graining, executed by Anthony Goth, of Bethlehem, Pa., was exhibited by Mr. Jones.

The appearance of the natural grain in these specimens, is so admirably imitated as to almost defy detection, and affords ample testimony to the truth of the inventor's assertion, that the process is the most perfect yet imitated. Mr. G. has given no information as to what the process is, there not having been sufficient time before the exhibition night to prepare the tools.

ERRATA.

In S. W. Robinson's article on *Suspension Bridges*, in the number for September, 1863, page 146, under the radical of second equation preceding equation (1), there should be $-2\pi\cos a$, in lieu of $-\pi\cos a$.

Page 147, the first term of equation (9) should be $\frac{5}{2}$ power, instead of $\frac{3}{2}$.

Page 149 equation (17) should be—

$$\left[1+\left(\frac{x}{2y}\pm\sqrt{\left(\frac{x}{2y}\right)^2-2}\right]^{\frac{3}{2}}=\left[\frac{x}{2y}\pm\sqrt{\left(\frac{x}{2y}\right)^2-2}\right]^2\frac{\rho_o}{2y}.$$